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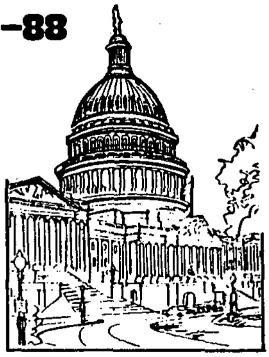

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ARTICLES

THE GROWTH OF DEMOCRATIC LEGITIMACY IN SPAIN

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The political transition in Spain provides a rare opportunity to monitor popular attitudes toward alternative regimes. Through the analysis of national surveys conducted in 1978, 1979-80, and 1984, we first establish that the Spanish public distinguishes not only between successive governments—the Franquist and the center-right and socialist governments of the post-Franco period—but also between Francoism and democracy as political systems. Second, we show that during the post-Franco era the criteria of legitimacy have begun to shift from formal political to social democratic values. These analytical results are achieved by comparing standard with less orthodox measures of political legitimacy and performance, and by revising conventional theories of system support. Third, we estimate the determinants of support for and opposition to the two regimes. The Franquist system remains more polarizing than does the democratic system; the constituencies of the democratic regime are considerably broader and more heterogeneous. However, while the new democratic state is comparatively inclusive and autonomous, low rates of political participation and changes in traditional socialist ideology have made the institutional bases of legitimacy ambiguous.

Spain's break with authoritarianism since the death of Franco in 1975 has been substantial and on occasion dramatic. The democratic regime has survived the onslaughts of regional separatists and of reactionaries in the military. How legitimate, then, is the new political system? The transfer of power from a center-right party, the Union of the Democratic Center (UCD), to the center-left socialists (PSOE) that took place in the general elections of 1982 makes it possible, in principle, to disentangle support for government incumbents—the authorities of the hour—from commitment to the presumably more basic regime or political system. The succession of governments gives us a chance to distinguish between Francoism and democracy.[1]

A primary objective of our study is to determine whether the split between Francoism and democracy holds up in the perceptions of Spaniards themselves. After the euphoria of the early days of the transition, the citizenry may have settled

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back into its proverbial cynicism about *los de siempre*, the powers that be, without making any meaningful distinction between new and old leadership and institutions. Such an outcome is hardly unthinkable, given the slide in economic performance and the rise in street crime during the post-Franco period. "Against Franco," the wisecrack went in Madrid, "we lived better."

Theoretical Perspectives

Most approaches to political legitimacy make two related distinctions. One is between system support based on affective, diffuse commitment and governmental popularity based on policy effectiveness or performance. This distinction derives from a split in the criteria of support: the former is thought to be deep and slow to change, the latter shallow and more labile. Another, partially separate, distinction concerns the objects rather than the criteria of support. It postulates a threefold division among the authorities, the regime, and the community.

In bare outline, this is the foundation of mainstream theorizing about political legitimacy (Easton, 1975, 1976; Lipset, 1960). Empirical confirmation, however, has been elusive, and in Spain we encounter an anomaly that contradicts the theory.[2] On the one hand, presumed measures of system support vary across governments. The socialists elicit much greater trust than did the center-right government that preceded theirs, yet the regime within which both governments have operated is the same. On the other hand, indicators that supposedly tap evaluations of performance in economic and social as well as political areas separate the Franco and post-Franco regimes from each other without revealing sharp differences between the post-Franco, democratic governments. These results challenge the received wisdom about political legitimacy. Neither the "criteria" nor the "objects" guidelines work out.

The first step in solving this puzzle entails a recognition that legitimization in Spain may follow an evolutionary dynamic that reflects a shift from political to social standards. The change in priority from formal, or political, democracy to social democracy has been documented elsewhere in Western Europe (Marshall, 1964; Weil, 1985). This movement helps account for the caution with which the Spanish public treated the socialists in the early days of the political transition, and the greater enthusiasm accorded to the socialists once the democratic regime had been consolidated. There are problems with viewing this trend as inexorable: the socialists have come to power when socialism as an economic doctrine has gone out of favor. Nevertheless, it serves to substantiate a crucial theoretical point: the values on which legitimacy is based may shift within the same regime.

The second step begins with the unexceptional idea that in polities such as Spain—or Argentina and Brazil, for that matter—where regime change has occurred within the living memory of most citizens, the public is likely to distinguish between types of political systems, and not just among different incumbents and governments within these systems. Dichotomies between regimes overshadow possibly finer distinctions, based on multiple criteria and dimensions, within regimes. On the contrary, in polities where no regime change has taken place recently, contrasts between the legitimacy of different regimes make almost no sense—alternative polities are virtually impossible to imagine (Sniderman, 1981). In this case citizens are likely to discriminate among different governments according to performance criteria.

This line of argument has its limitations, too. One stems from the possibility that an overarching sense of legitimacy may decay, rather than accumulate, over time as the "common enemy" (e.g., Francoism) recedes into the past. Still, it helps resolve the paradox of the failure of supposed indicators of performance and policy effectiveness to distinguish between evidently different governments, when the overriding contrast is between rival political systems, both of which most citizens have lived through.

As the parallels with Brazil and Argentina suggest, the theoretical implications of our analysis are not unique to the Spanish experience; for this reason, the logic of the analysis needs to be kept plainly in view. While we take the tripartite division across political authorities, regime, and community as potentially verifiable, we depart in several ways from the methodological procedures it implies. The most conspicuous is by way of innovation in measurement, but the most fundamental revision is conceptual. Imagine the authorities-regime-community continuum as the column axis in a matrix where the rows are formed by three functional dimensions: political, economic, and social (McDonough, 1981; Walzer, 1983). Suppose, further, that Spaniards evaluate the government, the regime, and possibly the community by these different standards.

Now, the democratic regime (if such an entity exists in the eyes of the Spanish public, apart from the governments that make it up) might be perceived as legitimate in *social* terms—for example, as reducing, or properly trying to reduce, the disparities between rich and poor. In this case the standard would be one of equity or fairness (Tyler, 1984). All the same, it might be regarded as a disaster by *political* standards—e.g., for failing to maintain law and order. Similarly, it might be viewed as efficient along *economic* lines, for promoting economic growth, while failing by one or both of the other criteria. For example—and to complicate matters further—the new regime might be judged as adequate by both social and economic standards, while it is seen as no more, or perhaps even less, legitimate politically than the Franquist system.

Thus, legitimacy can be understood according to its range, or scope, as well as by depth, or levels. In effect, it is multidimensional. The political component of the "system" may be legitimate, while the economic and/or social components are not, or are only partially so, and vice versa (Dahl, 1985; Vidal-Beneyto, 1984). The putative compartmentalization of the dimensions of legitimacy is an empirical as well as a theoretical question, as is the supposed separation of a shallower government from a deeper regime.

In one respect, our focus on the variable dimensions of legitimacy is not particularly novel. It systematizes and makes explicit earlier treatments of the phenomenon that were occasionally casual and abstract. In another sense, however, the approach is controversial. The differentiation and, some might argue, the proliferation of multiple criteria of legitimacy run the risk of confusing indicators of regime affect with measures of government evaluation. It is difficult to settle such matters a priori without falling into inconsequential taxonomical refinements. Our provisional solution is to estimate the overlap or separateness of the presumed system and apparent performance indicators by calculating the associations between them.[3]

A final question remains to be resolved. Although the foregoing argument might be a plausible addition to the customary authorities-regime split, it does not apply to the level of political community as generally understood. In fact, empirical studies of political legitimacy have ignored this level, and have focused instead on the authorities-regime dichotomy, with uneven results. However, it is unrealistic to ignore the distinctive and, conceivably, alternative political communities or subcultures in Spain. Regional groups that reject incorporation under a Spanish nation, much less a sovereign national government, provide the most familiar examples; elements on the extreme left and right furnish others.

The possibility of regional and ideological disaffection raises a general question: if differences between regimes, beyond this or that set of authorities, can be detected, what are the social bases of system and antisystem sentiment in the larger community? The first part of the question is a function of theoretical precision and definition, and a successful response to it depends on careful measurement. The second part of the question assumes that the first has been answered, and focuses on the determinants or the demographics of legitimacy.

Estimating the links between legitimacy and its predictors is fairly straightforward. Indicators of system support stand on the left side of an equation in which the right side is made up of indicators of various divisions in the community—for example, region, class, rural/urban, and so on. Still, the analytical implications of differing empirical outcomes need to be anticipated. Two theoretical traditions, one dealing with the relative autonomy of the state, the other with civil religion, help place the quantitative results in perspective.

The notion of civil religion, originating with Rousseau and elaborated most recently by Bellah (1970), is associated with an ideal of political community that is autonomous from parochial and primordial loyalties, such as religion and region. Not exactly a crude patriotism, nor necessarily an ideology of statism, it reflects the evolution of a political culture that grants the public sphere a life and merit of its own. In the European and American experiences, civil religion has been most nearly approximated by the republican virtues represented by the solid citizens of Renaissance city-states and by the founding fathers of the United States (Bellah and Hammond, 1980).

Civil religion implies a positive political model in the moral sense that the public sphere is looked upon not only as autonomous, but as the setting for education in political virtue and civic responsibility (Almond and Verba, 1963; Fliegelman, 1982). Theories of the autonomy of the state make no such claim. The fundamental notion is that "the state"—an entity virtually identical to "the regime"—enjoys medium- and probably long-term policy discretion vis-à-vis the classes and interests ("the community") supporting as well as opposing it (Evans, Rueschemeyer, and Skocpol, 1985; Nordlinger, 1981). The state apparatus may operate for some time in a practically autonomous manner, whether or not its decision-making discretion is enhanced by cultural hegemony—that is, by a "civil religion."

The relevance of these ideas to the understanding of political legitimacy in Spain is reasonably clearcut.[4] An important criterion of legitimacy is the extent to which the new regime, as compared to the Franquist system, is autonomous in the sense that it is not beholden to a narrow cluster of class interests or regional or religious communities. Empirically, this would be reflected in a relatively low polarization associated with the democratic regime, as compared to the strong polarization surrounding Francoism. In short, the new regime should not be strongly identified with any particular class, group, or community. It should be seen as more impartial, and its support less narrow, than the system that preceded it.

Such an outcome is mysterious if it is assumed that only authoritarian governments can approach autonomy, by reason of their supposed "strength." Conversely, democratic government, being "responsive," might be characterized as less autonomous, overloaded by "special interests." The trouble with this line of reasoning is that it presupposes a thorough incomparability between the exclusionary, restricted nature of Francoism, and indeed of most authoritarian systems, and the inclusionary and almost certainly more heterogeneous bases of democratic regimes (Epstein, 1984). Our position is simply that inclusiveness as a basis of legitimacy in the democratic case is to autonomy what "strength," or force, is for authoritarian regimes.

Legitimacy in these terms—in effect, as autonomy, whether democratic or authoritarian—is still circumscribed. The fact that the new regime may be more socially encompassing, less politically restrictive, and more autonomous than Francoism is an important component of legitimacy. The distance between the new regime and its social bases constitutes a slack resource that probably gives political leadership room for maneuver, but this flexibility—the reverse of the ideological and dogmatic style long identified with public life in Spain—is not itself a political philosophy, and it is not equivalent to a civil religion (Aumente, 1985b). The legitimacy of democracy in Spain may be constrained not only by the inexperience with mass politics ensuant on 40 years of Francoism, but also by the fact that Spain is renewing its democratic venture at a time when trust in and affiliation with the mainstream institutions of democratic politics, such as parties and trade unions, are stagnant or falling off, and when the social democratic, Keynesian suppositions—the surrogate civil religion —of the postwar period are suffering a crisis of credibility (Berger, 1981; Sotelo, 1984).[5]

In summary, the new regime has broken free of the essentially parochial and sectarian props of Francoism; the scope of its support is wider and more varied. It is less polarized and more diffuse. This inclusiveness is an indispensable feature of democracy, as contrasted with authoritarianism.

However, whether the Spanish democratic system has developed a political vision or culture of its own to incorporate diverse constituencies into a coherent political community remains problematic. A potential cost of depolarization is a lapse into political privatization, with low rates of involvement in public affairs and an uncertain commitment to democratic procedures (Bellah, Madsen, Sullivan, Swidler, and Tipton, 1985; Habermas, 1984; Keane, 1984). By itself, the ambiguity of political values in Spain is not a symptom of rampant anomie, since much of it reflects a new-found ethic of toleration; in the Spanish experience the alternative to such "relativism" has been composed of variations on "maximalism," "triumphalism," and related crusades that involve killing for an ideal. Yet, when considered alongside levels of political involvement and participation that seem low even after allowances are made for the fledgling nature of Spanish democracy, this inchoate pluralism forces us to contemplate what institutionalized legitimacy means for ordinary Spaniards.

The conceptualization of political legitimacy presented here is unorthodox, and it is well to lay out how the ideas will be tested. The analysis proceeds in two principal stages, with an epilogue. The first task is to define and detect—to "operationalize"—legitimacy. This entails determining whether Spaniards view Francoism and the democratic governments following it as two distinct regimes, as essentially the same, or as nothing more than a series of different governments, without distinguishing genotypes. We accomplish this by examining the extent to which Spaniards perceive the political systems as coherent wholes, differing more from one another than within themselves on grounds of economic, social, and political performance. The basic finding is that the Spanish public discerns a bipartite split between Francoism and democracy.

With the evaluation of regimes sep-

Table 1. Measures of Political Affect and Institutional Support: Marginal Distributions (in percentages)

	Trust Government?		Government Favors:		Tax Money Is:		
Year	Yes	No	Many	Few	Spent Well	Partly Wasted	Wasted
1984	41	59	58	42	23	48	29
1980	26	74	32	68	12	44	43
1978	27	73	35	65	12	47	41

	Elections		Congress		Monarchy or Republic?[a]		
Year	Needed	Not Needed	Needed	Not Needed	Republic	Indifferent	Monarchy
1984	91	9	81	19	—	—	—
1980	94	6	86	14	23	56	21
1978	93	7	87	13	32	42	26

Note: Sampling and question wording information is provided in Note 6.

[a] Not asked in 1984.

arated from the perception of mere governments, the second stage is to estimate the inclusiveness of the Franquist and democratic systems. This operation involves assessing the degree of polarization surrounding each regime—that is, the degree to which they are seen as incorporating particular groups and distinctive subcultures at the cost of marginalizing others. As a practical matter, the test means determining the weight of socioeconomic predictors such as class, region, and religion in accounting for variation in support for and opposition to the regimes.

Finally, we explore the possibility that political autonomy, treated as the comparative freedom from ties with distinctive social clusters and subcultures, is only a partial indicator of legitimacy. This exploration is devoted to a tentative analysis of what might be the emerging bases of a positive political framework in Spain.

Standard Measures of Legitimacy

Table 1 presents the univariate distributions of some traditional indicators of system support across three points in the post-Franco period: in 1978, during the first term of the center-right government led by Adolfo Suárez; in 1980, during the second term of the Suárez government, when what Spaniards call a certain *desencanto* (disenchantment) with the democratic process appeared to have set in; and in 1984, two years into the socialist government headed by Felipe González.[6]

Rudimentary as they are, the data furnish evidence of both stability and change. The consensus favoring democratic institutions—the need for elections and for the preservation of representative politics through the Cortes—is overwhelming and steady. However, variation in political affect—for example, in trust and in perceptions of the majoritarian versus elitist inclinations of the government—is considerable. A sharp break occurs between 1980 and 1984, between the UCD and the PSOE governments. The jump in positive affect coincides with the incumbency of the socialists.

None of these results corroborate expectations about crisp demarcations be-

Table 2. Political Affect and Institutional Support by Party Identification (in percentages)

	Party Identification[a]						
Indicators	PSOE	PCE	None	CDS (UCD)	Right	Local	Other
Trust Government							
1984	60	39	35	25	23	23	16
1980	18	13	29	44	25	16	2
1978	20	14	28	53	22	23	16
Government for Many							
1984	75	47	53	55	36	44	39
1980	26	10	34	60	40	16	13
1978	27	14	36	65	41	18	20
Taxes Well Spent							
1984	38	18	17	9	13	13	8
1980	7	5	12	32	21	7	3
1978	6	2	13	29	14	5	9
Elections Needed							
1984	96	93	89	90	84	92	87
1980	97	94	93	95	81	94	85
1978	96	93	93	96	85	97	85
Congress Needed							
1984	85	83	78	84	71	83	79
1980	91	89	84	88	79	81	79
1978	91	85	86	92	69	91	78
Favor Republic							
1980	29	65	14	2	6	42	76
1978	37	74	18	8	6	31	58

Note: Information for question wording and sampling is provided in Note 6.

[a] The CDS is a small centrist party with some resemblance to the once much larger and now defunct UCD. The PCE is the Community party of Spain. The "right" included mainly the Alianza Popular, with a few adherents of the Fuerza Nueva. "Other" indicates the primarily extraparliamentary mini-parties of the left.

tween government and regime or between popularity and legitimacy. Table 2 provides further evidence that shifts in the traditional indicators may be tied closely to alternation in government rather than to regime change. The measures of institutional and affective orientation are broken down by partisan identification.

The partisanship of Spaniards strongly influences their supposedly generic political views. With the socialists in power, 60% of the PSOE supporters indicated trust in the government. When the socialists were in opposition, the corresponding figure among PSOE partisans was about one-third as large, roughly 20%. Conversely, under the reign of the UCD, about 6 out of 10 UCD identifiers said that the government was "for the many," while only about 1 out of 4 socialists had the same opinion. Rather than tapping longstanding dispositions, fluctuations in these indicators seem to be quite contingent on specific incumbents.

However, another explanation is also compatible with this evidence, even though it is not plainly superior to the logic of reduction-to-political-circumstance. The sharp increase in esteem for the socialists may reflect a change in the bases of support, from liberal-political to social-egalitarian values. The fact that commitment to the institutions of pluralism holds up at a constantly high level, while trust and the other indicators evidently tied to the fortunes of the socialists soar, contraverts zero-sum interpretations of the trade-offs between the versions of democracy. Whether "support" entails legitimacy, popularity, or some mixture of the two, remains indeterminate.[7]

The inferential ramifications of the standard measures are not always negative or ambiguous. The political orientations of those who identify with conservative parties—mainly, the Alianza Popular—are very stable, relative to those of Spaniards in the center and toward the left. It would be misleading to conclude that the Spanish right maintains a uniformly dim view of democratic institutions and values. Rather, they do not distinguish as sharply as others between the center-right and the socialist left. Hence, their trust in and assessments of the fairness and efficacy of the government remain for the most part unaffected by the changes in government that have occurred so far.

Nevertheless, the broader lesson of these first-cut results is that we are not going to get very far with the analysis of system support by sticking to conventional indicators of the construct or, indeed, by leaving standard theories of legitimacy intact.

A Revisionist View of Political Performance and Legitimacy

Table 3 traces the movement in several indicators of political orientation, all of them scored between 1 and 10, across the time points plotted earlier for the traditional indicators. The new measures are of three types. Toward the top of the table are generic indicators, such as satisfaction with "the way democracy is working nowadays in Spain." The next set taps perceptions of how well different governments, including the Franco government "during its last ten years," did or have been doing along three dimensions: economic ("improving the standard of living"), social ("treating Spaniards from different social classes equally"), and political ("maintaining law and order"). A third group of indicators is composed of popularity ratings for major figures in Spanish politics, including the Pope and Juan Carlos, the king; the reasons for introducing these variables will be set forth momentarily.[8]

Two trends should be noted at the outset. The first is the recovery of assessments favoring democracy and the governments and leaders associated with it, after a downturn at the beginning of the 1980s. One of the most striking patterns is the recuperation of perceptions regarding how well democracy is working. They reach a high of 5.5 in 1984 after having fallen to 4.1 in 1980. A similar upswing can be detected in the popularity of leaders whose fortunes have been associated with the new regime—notably, Suárez and González.

It is not difficult to come up with a plausible explanation for these curvilinear trends. In the face of a deteriorating economy, mounting terrorism, and increasing crime, Spaniards began to exhibit a certain fatigue with democracy toward the beginning of the 1980s. Their spirits lifted with the coming-to-power of the socialists, with the implicit hope of better economic as well as political times, and, even earlier, with signs that terrorism was waning (Cebrián, 1985).

A second set of trends is linear but more complex. There has been a steady decline in the popularity and the fortunes of the communist left. Santiago Carrillo resigned as leader of the party, to be replaced by a younger militant, Geraldo Iglesias, whose following seems no greater than that of his mentor (Paramio, 1985a). An almost obsessive moderation has marked the transition to democracy in

Table 3. Average Scores for Selected Indicators of Political Orientation, 1978–1984

Indicators	1978	1980	1984
Satisfaction:			
With Democracy	4.45	4.19	5.54
With PSOE Government[b]	—	—	5.04
With UCD Government	4.52	4.17	4.42
With Franco Government	3.58	3.74	4.13
Living Standards:			
Under PSOE Government	—	—	4.97
Under UCD Government	4.26	4.04	4.24
Under Franco Government	3.94	4.31	4.47
Law and Order:			
Under PSOE Government[b]	—	—	4.70
Under UCD Government	4.24	3.18	4.25
Under Franco Government	4.94	5.15	5.31
Social Equity:			
Under PSOE Government[b]	—	—	5.04
Under UCD Government	3.82	3.62	4.27
Under Franco Government	2.82	2.93	3.77
Popularity:			
The King	6.44	6.50	7.97
John Paul II[c]	—	6.79	6.92
González	5.60	5.45	6.50
Suárez	5.42	5.08	5.37
Carrillo	4.30	3.94	3.26
Iglesias[d]	—	—	3.32
Fraga	3.27	2.94	3.68

Note: Information regarding survey is provided in Note 6.

[a] Responses were scored on a scale of 1–10, with 1 being the lowest score and 10 being the highest.

[b] The PSOE (Socialist Party) did not assume power until 1982; its governmental ratings were therefore not elicited until the 1984 survey.

[c] Omitted in 1978.

[d] Geraldo Iglesias succeeded Santiago Carrillo as secretary-general of the Communist Party of Spain in 1984.

Spain. Despite its protestations of Eurocommunism, the PCE has been tainted by a simultaneously extremist and manipulative history, and has been unable to shake its image as the repository of the antidemocratic left (Mujal-Leon, 1983).

By contrast, memories of Franco have become rosier, the farther the dictator fades into the past. The absence-makes-the-heart-grow-fonder effect is most obvious when it comes to assessments of law-and-order accomplishments. Neither does Franco's absolutely low, but still rising, grade with respect to social equity require much explanation. The rudiments of a welfare state were built during the last decade of Francoism; to many, the job tenure and medical benefits of that period compare favorably to post-Franco unemployment rates and the hazards of the underground economy (Flora and Heidenheimer, 1981; Lancaster, 1979). Furthermore, if recent economic conditions in Spain are set against the boom times of the later Franco period, his prosperity rating is also understandable.

For all this, with the exception of the law-and-order criterion, the ratings of the socialists still exceed those given to Franco, even if his image continues to improve with age.[9] The two regimes are rated differently by Spaniards, as are the center-right and center-left governments of the democratic regime itself.

Nevertheless, the creeping nostalgia for the good times, if not the politics, associated with the Franco period, and the fluctuating esteem in which post-Franco governments have been held place the legitimacy of the new political system at issue. What do the measures reviewed so far reveal about the solidity of democracy in Spain?

The simplest way to proceed is to see how the indicators hang together—the degree to which they are correlated—in light of theoretical expectations about the links between the areas of governmental performance and the dimensions of regime legitimacy. Consider the standards used for evaluating each of the governments—Franco's, the center-right UCD, and the center-left PSOE. In the present case, four main outcomes of a data-reduction technique such as factor analysis are possible. The least likely is that no significant differences emerge across either the three governments or the three criteria of evaluation. The outcome is improbable in view of the evidence presented in Table 3, which shows that the governments and their functions are perceived differently.

Another, more intriguing and perhaps likelier outcome would be reflected in the emergence of two or three distinct functional factors that do not, however, discriminate among the different governments. Such a configuration might lead to the inference that while Spaniards are quite capable of placing greater or lesser emphasis on, say, social equity as compared to economic growth, they see virtually no difference between governments in the implementation of these goals. This pattern would also imply that the governments—and regimes—are epiphenomenal, and that it is differences in the priority they attach to economic and social goals, together with their success in reaching them, rather than their democratic or authoritarian "processes of decision making," that determine their legitimacy. System support, such as it is, is driven by performance and effectiveness (Anderson, 1970).

There is a third, quite different pattern that seems at least equally probable. This would occur if Spaniards distinguished sharply among the three governments, with the implication that they perceived each of them serially, as separate policy and leadership packages. The pattern would demolish notions of regime legitimacy as something separable from support for particular governments, at least with the indicators at hand. Finally, results along these lines would reinforce the suspicion that the indicators themselves are unlikely measures of system support, but instead simply register perceptions of efficiency in different policy spheres.

By now the contours of the fourth major outcome should be evident, This would entail a division not among the three governments, but between the two, "qualitatively different" regimes, with the Franco regime on one side and the combined UCD/PSOE regime on the other. Only under this configuration would the typological separation of authoritarianism and democracy be substantiated empirically.

The data confirm the fourth outcome. Table 4 displays the results of a factor analysis of perceptions of economic, social, and political performance for the Franco regime and the two post-Franco governments.[10]

The key result is that two, and only two, dimensions emerge: one for Franco and one for the combined democratic governments. What does not happen is equally significant. Three separate factors for the different governments do not

Table 4. Oblique (Promax) Factor Matrix with Standardized Loadings: Franco, UCD, and PSOE Evaluations, 1984

	Factors[a]		
Evaluation Indicators	Authoritarianism	Democracy	Communalities
Franco:			
Standard of Living	1.01	−.11	1.02
Social Equity	1.00	−.05	1.01
Law and Order	1.01	−.11	1.02
UCD:			
Standard of Living	.52	.80	.91
Social Equity	.49	.82	.91
Law and Order	.39	.88	.92
PSOE:			
Standard of Living	−.24	.96	1.09
Social Equity	−.25	1.00	1.05
Law and Order	−.43	.95	1.09
Sum of Squares	4.11	4.92	

Note: Information regarding survey is provided in Note 6.

[a] Correlation between factors = .11.

appear, nor do we pick up stray factors associated with one or another of the functional dimensions. The authoritarian and democratic regimes are viewed "in the round," as not only more different from each other than particular governments might be, but also than any similarities in specific functions or policy areas might suggest.

All the same, it is important to recognize that the UCD government, while tied, together with the PSOE government, to the democratic regime, is perceived as being genuinely transitional. As the loadings of the UCD government on the Franco factor indicate, it leans a bit toward the authoritarian dimension, despite the fact that its center of gravity is with "democracy"—in effect, coalescing with the PSOE. Thus, even though the center, at least as incarnated in the UCD, has disappeared in Spain, the public understands that the UCD carried out a crucial role in the early days of the transition.

The data also allow us to view the nature of the transition, still from the viewpoint of the Spanish public, from a more synoptic perspective. We can chart the correlations between the authoritarian and democratic dimensions that emerge from factor analyses conducted on the 1978 and 1980 as well as the 1984 data (McDonough and López Pina, 1984). The purpose is to see how separate or close the two dimensions become over time and, by extension, to estimate how smoothly or abruptly the transition from Francoism to democracy unfolded in the eyes of ordinary Spaniards.

For 1978 the figure was .49, and in 1980 .38. The 1984 correlation, reported in Table 4 is .11. The steady decline in the correlations suggests (*a*) that, certainly in its initial stages, democratization was seen as genuinely transitional by many Spaniards, rather than as a sharp break with the past, and (*b*) that over time the democratic regime has taken on a life of its own, becoming progressively dissociated from the Franquist past.

In summary, it appears that Francoism

and democracy are perceived not only as distinct regimes but, reflecting the fairly smooth transition, as regimes that become progressively differentiated with the passage of time. To be sure, the Spanish public distinguishes among actual governments—for example, between the UCD and the PSOE incumbents. Nevertheless, Spaniards also discriminate, at a deeper level, between the Franquist and democratic regimes. This is what the factor analysis, which seeks underlying dimensions, evidently succeeds in demonstrating.

The obvious objection to this conclusion is that the indicators do not measure legitimacy at all, but continue to reflect mere performance criteria. In some quarters factor analysis has a faintly alchemical air. Whatever the statistical results, an interpretation that opts for a system-support reading of the data depends to a great extent on the validity of the measures used as input to the analysis. In the following section we shall argue that this objection becomes difficult to sustain to the degree that reified divisions between political legitimacy and efficiency are relaxed.

The essential point, however, is that historical circumstances surrounding the Spanish transition strongly condition the analytical results. The proximity in time of the two regimes tends to submerge differences between the post-Franco governments that would otherwise be more prominent. In another setting—say, Spain 20 years hence, or almost any of the present-day polities of Western Europe that reestablished their democratic institutions after World War II—differences across governments, based on the evaluation of performance, might come through unequivocally. The appearance of differences between regimes, however, would be most improbable. Such a possibility seems to require the experience of, or the opportunity to imagine, alternative political systems (Aumente, 1985a).

The Phases of Legitimacy

The common thread running through the analysis presented so far is that the data cannot be interpreted in a narrowly positivistic manner, as if "system support" and "legitimacy" were timeless Platonic abstractions or ideal types impervious to reasonable measurement or, indeed, beyond the grasp of Spanish citizens. The fact that the context of our study of regime support is a transition from authoritarianism to democracy, rather than a setting in which democracy has been long established, shapes the meaning of the empirical results.

Our argument rests on two developments: the historical contrast between the Franco and post-Franco regimes, and an evolution, within the democratic regime, in the bases of system support. The interregime contrast and the intraregime dynamics can be understood as successive phases of legitimization. Table 5 documents some of the links between these phases. It presents the correlations, for each of the three surveys, between the standard measures of political assessment used at the outset of the analysis and the summary indicators (factor scores) of perceptions of the Franco and post-Franco systems. The coefficients for satisfaction with the way democracy is working provide a validity check on the use of the "democracy dimension" constructed from multiple measures.

The democracy factor and the satisfaction with democracy indicator parallel each other in their size and stability. The magnitude of the correlations with the standard indicators is about equal, and the correlations hold steady over time. The correlations are positive, since the indicators all tap aspects, whether of the legitimacy or the efficiency, of democratic governments. The correlations with the institutional measures (the need for elections and congress) are uniformly modest, in large part because there is great consen-

Table 5. Correlations between Authoritarianism Factor Scores, Democracy Factor Scores, and Satisfaction with Democracy, and Indicators of Political Affect and Institutional Support

Affect and Institutional Indicators	Authoritarianism			Democracy			Satisfaction with Democracy		
	1978	1980	1984	1978	1980	1984	1978	1980	1984
Trust in Government	.18	.25	-.15	.45	.41	.47	.37	.40	.38
Government favors the many	.25	.29	-.11	.45	.41	.44	.36	.36	.36
Tax revenues spent well	.22	.23	-.17	.44	.42	.45	.37	.32	.36
Elections needed	-.06	-.09	-.12	.17	.17	.20	.15	.19	.20
Congress needed	-.09	-.09	-.14	.19	.22	.19	.18	.23	.21
Preference for Monarchy over Republic	.43	.38	—	.24	.18	—	.26	.16	—

Note: Description of survey is provided in Note 6.

sus—that is, little variation—surrounding these questions.[11]

The pattern displayed by the Franco factor is more complex. In 1978 and 1980, while the correlations of the Franco factor with the institutional measures are mildly negative, the coefficients related to such measures as trust in the democratic government are in fact positive, though not robustly so. This differential interaction between orientations toward Francoism and orientations toward democracy echoes what was suggested earlier: that the new regime was viewed as genuinely transitional, with links to the past, rather than as a venture in swift, radical change. However, by 1984, when the socialists had been in power for two years, the relations of the Franco factor with all the measures connected to democracy have become inverse. The coefficients are small, but their signs are consistently negative.

The change in the connection between orientations toward Francoism and toward democracy, as conventionally measured, must be understood in conjunction with the stability of the connection between orientations toward the democratic regime and the traditional measures of system support. The accession of the socialists to power reversed whatever links existed between Francoism and democracy. But the change of government did not alter the commitment of anti-Franquists to the democratic regime. On the one hand, the stability of the results buttresses the conviction that the data reveal orientations toward a political system that is in fact larger than specific governments, and that differs from the preceding regime. On the other hand, the change in the results, signified by the progressive separation between orientations toward Francoism and democracy during the later phase of the transition, suggests that the basis of legitimacy has expanded from liberal democratic toward social democratic norms. The perceived opening toward social democracy, or toward a variant of populism, is more threatening to Franquists than the earlier stage of the transition toward democratic capitalism under the aegis of the UCD.[12]

Table 6 presents the same data in a disaggregated form that makes it easier to spot the specific contrasts between and within regimes. It is composed of a series of analyses of variance in which the dependent variables are standardized scores on the authoritarianism and democracy factors.

One remarkable feature of these data is the confirmation they provide of the long-

Table 6. Analyses of Variance: Standardized Authoritarianism and Democracy Factor Scores by Political Affect and Institutional Support

Affect and Institutional Indicators	Authoritarianism 1978	Authoritarianism 1980	Authoritarianism 1984	Democracy 1978	Democracy 1980	Democracy 1984
Trust in Government						
Never	-.22	-.28	.21	-.66	-.49	-.78
Rarely	-.05	-.02	.04	.09	.06	-.09
Sometimes	.31	.24	-.14	.59	.46	.41
Always	.17	.47	-.21	.42	.57	.55
Eta²	.04	.06	.02	.24	.18	.25
Government of Many						
Few	-.19	-.20	.10	-.33	-.26	-.46
Many	.30	.36	-.10	.58	.51	.35
Eta²	.06	.08	.01	.20	.16	.19
Tax Revenues						
Wasted	-.27	.45	.21	-.49	.74	-.55
Partly wasted	.08	.00	-.04	.18	.14	.05
Spent well	.34	-.24	-.24	.75	-.36	.56
Eta²	.05	.06	.03	.20	.17	.20
Monarchy vs. Republic						
Monarchy	.51	.46	—	.24	.21	—
Indifferent	.03	.03	—	.10	.03	—
Republic	-.53	-.54	—	-.31	-.25	—
Eta²	.19	.15	—	.06	.03	—
Elections						
Need	-.04	-.05	-.08	.05	.04	.08
No need	.17	.26	.24	-.55	-.57	-.35
Eta²	.06	.01	.02	.03	.03	.04
Congress						
Need	-.09	-.08	-.28	.06	.08	.13
No need	.14	.14	.33	-.45	-.45	-.22
Eta²	.01	.01	.10	.04	.05	.04

Note: Description of survey is provided in Note 6.

standing hypothesis that Francoism was a rather nonideological and opportunistic cast of mind or "mentality," not as a full-blown set of political beliefs (Linz, 1964). While the measures of association between the democratic indicators and the Franco factor are significant, and while the form of the relationships is often in a positive direction, the covariance is generally very modest. Many of the Spaniards nostalgic over Francoism do not seem to be unduly exercised about the onset and settling in of democracy, even if their judgment of socialist democracy is on balance negative. While the two regimes are viewed as distinct, they are not seen as utterly discontinuous, either by Franquists or by Spaniards with warmer feelings toward the new system.

The single area in which Franquist Spaniards show polarizing inclinations is with respect to the monarchy vs. republic issue. At least in the abstract, they tend to be monarchists. Democratically oriented Spaniards are comparatively lukewarm on the question, although they lean toward the republican side. This asymmetry serves to highlight the pivotal role of the king in providing an assurance of continuity during the transition for a reluctantly democratizing portion of the public (McDonough, Barnes, and López Pina, 1981).[13]

Finally, it is possible to separate out one indicator that has at most a vestigial link with regime legitimacy. This is the item tapping perceptions of governmental spending efficiency. In 1978, both the authoritarian and the democratic factors tied in with positive assessments of the government's role in this area. By 1980, in the depths of the *desencanto,* both turned negative; not even democratically oriented Spaniards looked favorably on the government's budgetary performance. By 1984, the Franquists were also set against the government, now led by the socialists, while the "democrats" adopted a much more supportive attitude.

The flip-flopping of the tax indicator, which is clearly a measure of governmental performance, contrasts with the remaining measures that, in varying ways and degrees, distinguish between the larger political systems. A principal lesson of the analysis of the links between the "old" and "new" indicators is that rigid dichotomies between "legitimacy" and "performance," as well as analogous divisions between "qualitative" and "quantitative" observation, can be artificial. In the first place, the historical proximity of Francoism and democracy tends to overshadow niceties of evaluation based on performance. Second, given the basic demarcation between regimes, the amplification within the democratic system from political to social norms of legitimacy reflects a natural tension and ambiguity in the definition of democracy, rather than merely a preference for this or that set of incumbents. Third, either/or categorization of regimes, like the dichotomy between legitimacy and efficiency, is particularly suspect if applied to political change that happens to be transitional, not revolutionary.

Determinants of Legitimacy

Authoritarianism and democracy are separate regimes, and not just discrete governments, in the eyes of the Spanish public. What remains to be explored is the social bases of the legitimacy of the democratic regime.

Table 7 represents a first attempt to resolve this question. It assembles the correlations, for the three successive surveys, between the series of indicators introduced earlier and the factor scores tapping orientations toward the authoritarian and democratic regimes. The correlations convey a sense of the relative polarization surrounding the political systems. For example, if feelings of religiosity correlate strongly with positive assessments of Francoism, but only weakly with evaluations of the democratic regime, then one possibly crucial determinant of legitimacy from within the Spanish community, religious vs. secular sentiments, can be said to be polarizing for the Franquist system, but less problematic for the new system.

In fact, this turns out to be consistently the case for the religiosity indicator. Starting in 1978, the association between religiosity and Franco is .39, in 1980 .41, and in 1984 .38. By contrast, the corresponding coefficients for the democratic regime are .19, .10, and .06. Again, it is important to note what does not happen. While the correlations of religiosity with Franco are strongly positive (and the left-right/Franco coefficients are even higher), the figures associated with the UCD/PSOE regime are not reversed—that is, they are not strongly negative. Instead, they are simply washed out.

The results are much the same for the other indicators. The Franco regime elicits stronger passions, both positive and negative, than does the democratic system. In this sense Franco was a polarizing figure. But the phenomenon is not purely or even mainly psychological. Spanish authori-

Table 7. Correlations Between Scores on Democracy and Authoritarianism Factors and Selected Socio-political Indicators

	Authoritarianism			Democracy		
Indicators	1978	1980	1984	1978	1980	1984
Left/Right Placement	.60	.60	.57	.18	.13	-.12
Religiosity	.39	.41	.38	.19	.10	.06
Satisfaction:						
With Democracy	.13	.10	-.17	.81	.68	.57
With PSOE Government	—	—	-.26	—	—	.68
With UCD Government	.25	.22	.37	.91	.72	.57
With Franco Government	.94	.80	.85	.21	.16	-.05
Popularity:						
The King	.35	.33	.17	.37	.30	.31
John Paul II	—	.46	.43	—	.17	.09
González	-.10	.01	-.25	.30	.32	.52
Suárez	.32	.39	.14	.48	.42	.33
Carrillo	-.24	-.26	-.34	.15	.20	.18
Iglesias	—	—	-.33	—	—	.19
Fraga	.49	.51	.55	.14	.12	-.05

Note: Information regarding survey is provided in Note 6.

tarianism was demobilizing—that is, strongly exclusionary. Although it was probably not ideological in the fullest sense, it did evoke powerful defensive loyalties from its adherents, as well as hatred from those whom it defeated and resentment among those who had become marginalized by it. Once the constitutional roadblocks were overcome in the early days of the transition, political exclusion ceased to be a problem, and much of the combativeness of Spanish politics was defused, its raison d'être having diminished (Maravall, 1984; Medhurst, 1984; cf. Eckstein, 1984).

The correlations between the popularity ratings of selected public figures and the summary evaluations of the authoritarian and democratic regimes are given because these personalities are known to express ideological currents, or *tendances*, in Spain (Barnes, McDonough, and López Pina, 1985). The anchoring roles of the king and of Adolfo Suárez are manifest in the positive correlations of their popularity ratings with both the Franquist and the democratic dimensions. By contrast, the two most polarizing figures are Santiago Carrillo, ex-leader of the communists, and, on the right, Manuel Fraga, leader of the Alianza Popular. These men define the permissible extremes of the competitive establishment in Spain.

Felipe González, the leader of the socialists, is seen largely as the center-left figure he is. The one polarizing chord struck by González occurs in 1984, in relation to the summary scores for the Franco dimension: the authoritarianism-González correlation is −.25. The coming to power of the PSOE, beyond its role as an attractive opposition, aroused the potential for countermobilization on the right—a reaction encouraged by the disintegration of the center-right and substantiated by the increase in the vote for Fraga's right-wing forces in the same election (1982) won by the socialists. The sign of distancing between González and the Franquists matches the shift toward norms of socialist democracy as legitimizing symbols, once the political institution-

Table 8. Multiple Classification Analyses: Authoritarianism (Franco) and Democracy (UCD/PSOE) Factors, 1984

Authoritarianism Factor Scores as Dependent Variable		Democracy Factor Scores as Dependent Variable	
Predictor	Beta	Predictor	Beta
Religiosity	.30	Political interest	.17
Region	.18	Size of city/town	.15
Political interest	.15	Region	.12
Class identification	.13	Education	.11
Age	.09	Age	.10
Size of city/town	.08	Income	.08
Education	.08	Religiosity	.06
Income	.05	Class identification	.02
$R^2 = .24$		$R^2 = .05$	

Note: Description of survey is provided in Note 6.

alization of the new regime had been secured.

There is one trend, however, that surpasses in importance those tied to individual politicians and other public figures. This is the gradual dissociation of support for the democratic regime from satisfaction with what seems to be the mere effectiveness of democracy.

In 1978, the correlation between positive orientations toward the democratic government and satisfaction with "the way democracy is working in Spain" was .81. In 1980 it dipped to .68, and by 1984 it had reached .57. It is unexceptionable that the two variables are positively related. What matters is that the strength of the correlation has dropped over time. This suggests that the victories and defeats experienced by any single government are less and less likely to touch off feelings of alternate triumphalism and despair—the venerable manic-depressive style of Spanish politics—associated with the political system as a whole, the fate of the nation, and other similarly grandiose stakes.[14]

The data just presented, while suggestive, are limited to bivariate correlations, and it is difficult to assess the relative impact of, say, religiosity and class identification on commitment to and disaffection from the political systems. The evidence on display in Table 8 reduces this problem. It summarizes the results of two multiple classification analyses—essentially, multiple regression in which nominal predictors such as region are permitted—for the factor scores that crystallize orientations toward the Franquist and the democratic regimes.

The most important finding is the vastly greater percentage of variance accounted for in the Franco factor than in the democratic factor by the same set of standard socioeconomic predictors. The Franquist regime was tied more closely to distinctive constituencies—for example, to the culture of Counter-Reformation Catholicism (Alvarez Bolado, 1976; Payne, 1984; Tusell, 1984); to the Castillian zone much more than to regions on the periphery, such as the Basque country and Catalonia (García Ferrando, 1982); to the upper class more than to the workers (López Pina and Aranguren, 1976), and so on. It is the two classic cleavages of Spanish politics—religion and region—that most sharply delineate the mass foundations of the authoritarian state.

What is remarkable is not this confirmation of the exclusiveness of the social boundaries of the Franquist system, but rather the contrast with post-Franco governments. The constituencies of the democratic regime are much more heterogeneous. It is not region or religiosity that tops the list of predictors, but "political interest" and the "small town/big city," or rural-urban continuum. These variables are not the stuff of confrontation; they are not dividing lines that unequivocally demarcate political identities. Even if they were charged with polarization, political interest and the like do not approach the "primordial" or pre-industrial demarcators such as region in accounting statistically for support for and opposition to the UCD/PSOE governments, in the way that these indicators operate with respect to Francoism.[15]

Thus, the bases of support for the democratic regime are much more variegated—broader and more ambiguous—than is the case for the exclusionary system that preceded it. The democratic regime is less strictly tied to particular interests; in this respect it enjoys relative autonomy. While this is not legitimacy or consensus pure and simple, and does not guarantee governability, the depolarization of democracy in Spain constitutes a major achievement of the new political system. The nexus of antagonism is Francoism, now a fading memory (Santamaría, 1985). Democracy in the last quarter of the twentieth century does not pit conservative and progressive Spaniards against one another in the way that democracy did in the 1930s, or as the remnants of Francoism continue to do.

We have seen that Spanish politics is less polarized and more permissive than it was during the days of Franco. The "spread" of the democratic system is wide relative to the sharply delineated constituencies underlying the old system. We have also established that the two regimes, Franco's and the post-Franco system of UCD and PSOE governments, are seen as separate by the Spanish public, thus opening the way for the analysis of legitimacy instead of mere support/opposition with regard to particular governments.

Heterogeneity is not consensus, however, nor can it be equated with legitimacy. The tolerance implicit in the variegated composition of the constituencies of the new regime—the comparative absence of polarization—is not the same as positive legitimacy, although it may be an important prerequisite of civility. By itself, it does not constitute a political vision.

Detecting the positive elements behind the lack of polarization surrounding the new regime is theoretically as well as empirically problematic, and we can only suggest a route of investigation here. The question matters to the degree that civil comity, pluralism, and the like have not been notable virtues of Hispanic public life, compared with the ideological ferocity and winner-take-all style of the past (Sánchez, 1964; Ullman, 1971).

Our suspicion is that, if there are positive grounds for the legitimacy of the new regime, beyond the rejection of Francoism, they have to do with expectations of a deepening of democracy from political liberalization toward social fairness. Strong hints of such an evolution have already appeared in the expectations surrounding the rise of the socialists, and the ideal of neopopulism is not exceptional in Latin societies with traditionally steep stratification hierarchies, or in societies recently emerging from an agrarian heritage. In Spain, the two legacies merge.

Figure 1 furnishes a bit of data—it is only that—to support this line of reasoning. The dashed curve shows the percentage of Spaniards who feel that workers, the middle class, and the upper class have respectively "done better" over the first two years of the socialist government. It reflects the cynicism of the *los-de-siempre* view—the judgment that the workers have not done particularly well even under the socialists and that, although no one is perceived as doing especially well, the upper class has done least badly of all.

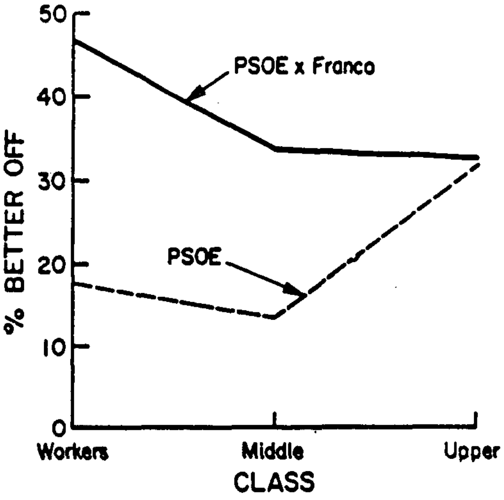
However, as the solid curve shows, when the question becomes how workers, the middle class, and the upper class have done under the PSOE government *compared with how they fared under Franco*, the picture changes substantially. A sharply inverted gradient emerges, with the workers on top, the middle class in the middle, and the upper class at the bottom. Spaniards do indeed distinguish between Francoism and the current political arrangement, and they do so partly in terms of equity considerations.

Another significant facet of the changeover is that it is not zero-sum. The upper class is thought to have done neither better nor worse in the contrast between the PSOE and Franco than it was thought to have done in the contrast within the PSOE. It is the middle class and, especially, the workers who are believed to have done better when the frame of comparison sets the present against the situation under Franco. The variable-sum properties of the interclass comparison reflect the diminished polarization under the new regime.

The lesson is reasonably plain. The Spanish democratic system, and the socialist government in particular, are not immune from substantial criticism. Short-term policy consensus is a poor standard of legitimacy. However, when the point of reference switches from the tribulations of day-to-day democracy to the equity of the system relative to its predecessor, the air of grumbling lifts, and a modicum of satisfaction is discernible. If Spaniards harbor a craving for an alternative to the present political system, that yearning does not imply a resurrection of Francoism.

Spaniards probably have a greater sense than they did under Franco that sacrifices as well as benefits are distributed more or less equitably. How long this sense of fairness can be maintained in the face of slow or even negative growth —less favorable conditions than prevailed during the rebirth of democracies during the postwar period in Western Europe— remains to be seen. The integration of Spain into the European community and the material advantages associated with this integration seem likely to bolster the democratic regime. In this scenario, political legitimacy would come to depend on, although it might not be reducible to, economic performance (cf. Lane, 1981).[16]

Figure 1. Percentages Claiming That "Workers," the "Middle Class," and the "Upper Class" Have Improved Their Situation (*a*) under the Socialist Government, and (*b*) under the Socialist Government as Compared with Franco's



Conclusion

The pragmatism of social democracy, its gradual loss of the radicalism and the vision of justice that had inspired it in the beginning, can be seen as a reaction against the excesses and the crimes of authoritarian and dogmatic socialism. This reaction has been salutary; at the same time,

> social democracy has been unable to fill the vacuum left by the failure of the great communist hope. Does this mean, as many predict, that the hour of the churches has come? If this should turn out to be the case, I hope that there will be left on the earth at least a small handful of human beings—as at the end of antiquity—who will resist the temptation of divine omniscience, as others, in our day, have resisted that of revolutionary omniscience. (Paz, 1985, p. 191)

The empirical study of political legitimacy has a checkered history. A primary reason for this has been the gulf between ambitious theorizing and the vicissitudes of measurement. The political transition in Spain has eliminated much of this difficulty by providing a rare opportunity to track popular attitudes toward alternative political regimes. Because the Franquist system is still vivid in the minds of most Spaniards, it has been feasible to ask questions about the legitimacy of authoritarianism and democracy that are not only conceptually important but also—and this seems to have been a major challenge in previous studies—meaningful to the man-in-the-street.

We have verified that the two regimes are perceived as separate, above and beyond the specific governments of which they have been composed. The measurement device on which the test was based is a set of items derived from theoretical expectations about the differential bases of legitimacy, and designed to elicit assessments of governments according to their economic, social, and political performance. The multiple indicators evaluating particular governments set up a series of logically distinct analytical outcomes. Only one of these outcomes—that in which the Franco and post-Franco governments were in fact divided into two separate regimes—would substantiate the existence, in the eyes of the Spanish public as well as in those of the theoretical observer, of authoritarian and democratic political systems. This turned out to be the empirical result. We then probed the socio-economic support structure of the new regime—that is, the demographics of its legitimacy. The natural constituencies of Spanish democracy are broader than the restricted subcultures on which Francoism was built.

The plausibility of our argument, however, depends as much on contextual factors as on statistical elegance. Fixed distinctions between legitimacy and efficiency, and between regimes and governments, turn out to be unhelpful, for three reasons. First, the historical contrast between the opposed political systems of authoritarianism and democracy is so vivid for Spaniards that it would probably remain salient no matter how differences between and within the regime were assessed. The contrast may fade in the future, as Francoism recedes into the past. But for now it is paramount.

Moreover, while the primary transition in Spain has been between authoritarianism and political democracy, the makings of a secondary transition in public opinion—from liberal to social democracy—can also be discerned. The shift is latent. It implies a change in the values by which post-Franco governments are likely to be evaluated, that is, an expansion in the norms of legitimacy. Although tentative, this evolution is visible beneath the movement away from the political restrictions of Francoism.

Third, the change from authoritarianism to democracy in Spain has been genuinely transitional after all, not revolutionary. The discovery of continuities between the regimes is therefore not surprising.

A major feature of the transitional nature of democratization in Spain is that the profile of support for the new regime is not the mirror image of the constituencies of Francoism. The democratic audience is large and diverse. In one sense, this pattern may seem obvious to the point of tautology. After all, in comparison with practically every authoritarian system, industrial democracies are mass democracies.[17] The point, however, is not only that the structural constituencies of democracy expanded with the political opening in Spain and the instantaneous enfranchisement of previously marginalized citizens. It is also the case that longstanding cultural as well as structural demarcators—in particular, religion and regionalism—have eroded as touchstones for mobilization and countermobilization. In this sense, the contours of the legitimacy of the new regime are more diffuse and more heterogeneous than the profile of support for Francoism. Spanish politics has become less polarized and more ambiguous than it used to be.

The variegation in the structural and cultural bases of Spanish democracy is central to an understanding of the political sociology of the post-Franco regime. It also raises questions about the nature of legitimacy under "post-modern" conditions (Anderson, 1984; Martín Santos, 1985; Villalonga, 1984). On the one hand, it is possible to argue that the new regime enjoys considerable autonomy from a narrow constellation of social forces and interests—a flexibility, however, that may be as much a function of the underorganization of secondary associations in Spain, compared to the strength of the state, than of the sheer heterogeneity of the regime's constituents (McDonough et al., 1984; cf. Useem and Useem, 1979). On the other hand, the multifaceted nature of its support raises the issue of how the regime will hold together, and the low rates of involvement in secondary associations cast doubts on the political socialization of the citizenry. The direction of the regime is further obscured by the timing of the socialists' ascent to power during a period of disenchantment with the shibboleths of social democracy, yet in the midst of popular expectations regarding economic well being.

The underorganization of Spanish democracy probably reduces the threat of the country's becoming ungovernable through a proliferation of interest groups, even if, simultaneously, the constraints imposed by economic hard times reduce the regime's ability to satisfy such groups. Prolonged economic stagnation may be overrated as an impediment to the consolidation of democracy in Spain. In the least, it is not the only impediment. Of equal importance may be the virtual absence of a normative bond, a political vision, to replace the accustomed "transcendentalism and triumphalism" of Spanish politics. "Principled" confrontation may give way, some analysts fear, to privatization and nihilism (Paniker, 1985; Paramio, 1984, 1985b).

The God-is-dead-Marx-is-dead-and-I-don't-feel-too-good-myself problem is not unique to Spain. The reformulation of the exhausted or threadbare ideological and material foundations of industrial democracy is also underway in Western Europe and the United States (Cebrián, 1984; Piore and Sabel, 1985). Part of the challenge of Spanish democracy is one of timing: it has been restored more than a generation after its peer nations revived their commitment to democracy. The stock solutions compounded of Keynesianism and party pluralism have been criticized and partially discredited, and a certain demoralization is detectable among those for whom social democracy was linked, in "normal" times, with economic growth (Altares, 1985; Offe, 1984). The increasing complexity of the Spanish electorate and the differentiation of Spanish society that have made political tolerance structurally possible may have rendered difficult the institutionalization of a political vision that can unify such diversity.

Notes

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1. Pluralities in the first two general elections under the democratic dispensation—in 1977 and 1979—were won by the UCD.

2. There is a considerable literature on political legitimacy that we do not utilize, mainly because it is empirically inconclusive and adds little to the formulations by Easton and Lipset. See, however, Hart (1978), Schaar (1981), and Wright (1976).

3. The chronic inconclusiveness of most studies of political legitimacy reflects, we believe, the disparity between elaborateness of theory and crudity of measurement. Furthermore, the fact that many standard indicators of legitimacy are nominal attributes may contribute to a certain petrification of concepts—for example, to mutually exclusive categorizations of values and attitudes that might be rendered more accurately as continua. This tradition may also work to confound "real legitimacy" with nominal classifications that are "fundamental," "longterm," "qualitative," and the like, as compared, invidiously, with scalar, quantitative measures that are assumed to register "efficiency," "performance evaluations," et cetera.

4. As with the question of political legitimacy, we omit a review of much of the literature on civil religion and on the autonomy of the state, for two reasons: it is theoretically ambiguous and empirically thin. Since we are not concerned with advancing the study of civil religion or of the autonomy of the state as such, our use of this literature is highly selective. For useful updates of the theoretical literature on the state, see Alford and Friedland (1985) and Benjamin and Elkin (1985).

5. Another factor behind the leeway enjoyed by post-Franco governments, in addition to the social heterogeneity and interclass nature of their bases of support, is itself a relic of Francoism: the continuing low levels of mass participation in voluntary associations (McDonough, Barnes, and López Pina, 1984). The organizational infrastructure for channeling conflicts is comparatively underdeveloped in Spain.

6. The surveys were conducted in June 1978, in December 1979-January 1980, and in November 1984. The number of respondents from the national population 16 years of age and older in each of the surveys is 3004 (1978), 3014 (1980), and 2994 (1984). The first two surveys were based on quota sampling at the individual level, with selection by categories of age and sex. Provinces, and then counties, were selected by probability-proportional-to-size criteria, as were *secciones* (roughly, "polling-booth areas"); then quotas by age and sex were drawn. Respondents for the third survey were drawn from an area probability sample, with random selection of households and individuals within households.

The trust in government, government for the few/many, and tax expenditure items were translated from the by now standard questions originally used in the National Election Studies conducted by the University of Michigan's Center for Political Studies (Miller, Miller, and Schneider, 1980). The elections question reads: "Do you believe that elections are the best system for choosing the government and the authorities of the country, or do you believe that they are not the best system?" The congress question reads: "Do you believe that we need a congress of deputies and a senate or could we get along without them?" The remaining question reads: "Do you think that Spain should be a monarchy, or that Spain should be a republic?"

7. The question is taken up in detail in the section on "The Phases of Legitimacy."

8. Except for the popularity items, the questions were introduced in the following manner: "Now I would like to ask you about some things which you might be satisfied or dissatisfied about in our country. As you can see, a *1* on this scale means that you are totally dissatisfied or very displeased, and a *10* means that you are completely satisfied or very pleased." Then respondents were asked, "Are you totally satisfied or totally dissatisfied with the way in which democracy is working nowadays in Spain?" The interviewer then continued, "Now I would like you to compare, for several things (*en una série de cosas*), the present government with the previous UCD governments and that of the last ten years of Franco."

The political evaluation item has provoked controversy among political scientists, even though it does not agitate most Spaniards. The law-and-order slant was chosen, rather than a freedom-and-liberty focus, because on pretesting the former elicited variance, while the latter confirmed only the obvious.

The popularity items were introduced as follows: "We would like to know where you stand (*su postura*) with respect to some prominent figures in Spain today." The original wording of all questions is available from the authors on request.

9. Franco's ratings are affected by the demographics of aging. For example, in 1984, the correlation between age and overall satisfaction with the Franco regime is .26. The corresponding figures for the UCD and PSOE governments are, respectively, .15 and .08. Furthermore, it is clear that many younger Spaniards do not have any opinion of Francoism. Of the cohort aged 16-19 years, one-third give no opinion about the Franco regime. For the cohort aged 20-29 years the figure is 15%; for the cohort aged 30-39 years, 14%, and so on, until those 60 years of age and older, for whom non-opinion is only 9%. In short, the steady increase in favorable ratings of Franco can be deceptive unless it is understood that they tend to be concentrated in segments of the population that are dying off. On balance, demographic replacement is working in favor of the democratic regime.

10. An oblique rather than orthogonal rotation is used. The promax algorithm is a version of oblique rotation, performed after computing a varimax (orthogonal) rotation that gives evidence of correlation between dimensions (Mulaik, 1971, pp. 300-304). Factor analyses of the 1978 and 1980 data, which give essentially the same configuration as that reported here, are presented in McDonough and López Pina (1984).

11. The monarchy-vs.-republic issue is a case apart; it is discussed below.

12. This interpretation does not assume that mass values have shifted in favor of a rigorous socialism since the early days of the transition. No significant further movement toward the left has been detected in the Spanish public. Nor has the public's perception of the PSOE or of Felipe González moved from the extreme left to the center-left; it seems to have been at the center-left for several years, for the most part unaffected by the moderates' and social democrats' internal conquest of the PSOE, and subsequently the government (Gunther, Sani, and Shabad, 1985; Linz, Gómez-Reino, Orizo, and Vila, 1981).

Instead, our interpretation can be boiled down to two hypotheses, First, the timing of PSOE's ascendancy has itself been crucial. Popular ratification of the socialist label at the start of the transition, prior to the consolidation of political democracy, would have been "a provocation." With the passage of time, voting for the socialists became less risky, and in any case the moderate leftism of the PSOE had for several years corresponded to the modal location of the Spanish electorate. Second, while they stand to the left of, for example, the Alianza Popular, and while their position has been on the left since well before the electoral victory of the socialists, the preferences of PSOE supporters with regard to economic and social policy do not constitute a mandate for rapid structural transformation. Rather, they reflect a comparatively uncrystallized package of traditional statist paternalism, bewilderment at tax policy, and upward mobility for "the little guy" (McDonough, Barnes, and López Pina, 1986).

Finally, the disintegration of the center-right UCD into factionalism—in large part because it had no place to go after fulfilling its role in the political transition—contributed to the electability of the already popular center-left socialists (Gunther, 1985). But the socialists did not simply fill a vacuum created by the collapse of the UCD, regardless of ideology; the public has consistently located the PSOE to the left of center.

13. The passion that Franquists bring to the monarchy-vs.-republic issue is probably diagnostic of their attachment to the past, including its controversies. "Asymmetry" results from the tendency of non-Franquists to get more worked up about issues relating to the present and the future (e.g., tax equity).

14. The progressive dissociation between satisfaction with UCD/PSOE governments and satisfaction with democracy may seem to indicate a waning of commitment to the new regime. While the possibility cannot be ruled out, the statistical relation remains very high, so that the indifference interpretation based on these data alone would be alarmist. It is also plain that the connection between satisfaction with democracy and attachment to the Franquist system, never very strong, has become weaker, to the point that by 1984 the association turns negative, corroborating an antipathy between the authoritarian and (social) democratic orientations. The 1978 correlation is .13; in 1980, .10; and in 1984, −.17. This, in turn, suggests that a certain polarization remains in Spanish mass politics and that, given the appropriate conditions, the forces of hypermobilization and countermobilization could return (Sani and Shabad, 1983). Here it suffices to mention the obvious point that disengagement of approval of Francoism from positive evaluations of the new regime substantiates the inference that the Spanish public indeed distinguishes between the two systems, and is not merely accepting of practically any regime—a conclusion that might be drawn if attention were confined to the early period of the transition, when evaluative perceptions of Francoism and democracy were rather strongly correlated.

15. The MCA coefficients alone can be opaque in explaining the direction of relationships. None of the coefficients have signs, since predictors can be nominal attributes. Without going through a predictor-by-predictor account, it may be noted that the relationships work in expected ways. For example, as the beta of .30 indicates, orientations toward the Franquist system vary sharply (and linearly) with religiosity; the corresponding link of religiosity with the new regime is irregular and washed-out (beta = .06). "Region" (of residence) is the only less-than-ordinal predictor. On the whole, Basques and Catalans are the least favorable to Francoism. While it remains important, the center-vs.-periphery conflict is considerably more attenuated for the "democracy factor." For an exposition of the statistical technique, see Andrews, Morgan, Sonquist, and Klem (1973). For a more detailed empirical analysis see McDonough, Barnes, and López Pina (1985).

16. Conventional measures of legitimacy have stressed political trust and participation and the like over imputations of social equity and fairness, with the latter tending to be labeled as performance or policy indicators. The distributive rather than radically redistributive cast to the data examined here raises a question we explore elsewhere: to what extent do populist-paternalistic expectations surround the socialist government and perhaps the democratic regime itself (McDonough et al., 1986)? At least some portion of the distributive cast to Spanish public opinion seems to be traditionally precapitalist rather than ideologically anticapitalist.

17. A major exception was Argentina under Perón (Schoultz, 1983).

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TOWARD A POLITICAL EXPLANATION OF GOVERNMENT VOTE LOSSES IN MIDTERM BY-ELECTIONS

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By-elections in parliamentary democracies are remarkable for the almost invariable loss of support experienced by governments relative to the preceding general election. There has been, however, surprisingly little research into this phenomenon. Using Britain as a test case, this article goes some way towards filling this gap. It identifies and tests three political explanations of governments' by-election vote losses, which it labels the turnout, referendum *and* personal vote *models. No simple conclusion emerges, since different results are obtained depending on whether the models are treated as separate entities or their interplay is taken into account by integrating them into a single causal model. This latter strategy is argued to involve a superior conceptualization of the processes at work in bringing about governments' midterm vote losses.*

Since the world moved into economic recession in the mid-1970s it has become fashionable to talk of the "ungovernability" of modern democracies. Particular attention has been paid in that debate to the determinants of governments' midterm popularity, since a basic assumption of democracy in practice is that governments are able to carry out their duties and responsibilities more effectively when they enjoy high public esteem: popularity provides a moral and political authority that opponents find difficult to resist. This link has always been apparent in the fragmented governmental system of the United States, where popular presidents often successfully appeal directly to the public for help in overcoming congressional opposition to their policies (Ostrom and Simon, 1985). Although less obviously so, it is also a link that holds in parliamentary systems characterized by firm party discipline. Because such discipline is not rigid, party leaders find it easier to control their party when they, and by inference their actions and policies, bear the stamp of public approval (Norton, 1975).

Using opinion poll voting intention data, a substantial crossnational literature has grown up around the question of the economic determinants of short-term fluctuations in the level of government support (good summaries are Paldam, 1981, and Whiteley, 1984; see also Husbands, 1985). An arguably more reliable measure of government popularity, however, is its performance in midterm elections, since this is based on actual rather than hypothetical voting behavior. The best-known such elections are off-year congressional elections in the U.S. presidential system and by-elections in those parliamentary systems where there is no proportional representation electoral system to dictate the automatic filling of a casual vacancy in the legislature by the next person on the list. Yet little is known about the dynamics of government support in this type of election.

An obvious exception to this general observation would be the material on the decline of the president's party in U.S. off-year congressional elections (Campbell, 1966; Hinckley, 1967; Kernell, 1977; and Tufte, 1975). The insights of this literature are not necessarily applicable to parliamentary by-elections, however, since the two types of midterm election lack sufficient similarity to be directly comparable. In particular, off-year congressional elections are national events involving the simultaneous election of all 435 members of the House of Representatives, as well as of 33 senators. They also always carry the possibility of a change of party control in one or both houses of Congress. By-elections, on the other hand, are ad hoc events that rarely involve more than one or two of several hundred constituencies at any one time, while their outcome seldom jeopardizes the government's majority position in the legislature.[1]

By-elections can, nonetheless, have very real political consequences. A government's poor performance in them can erode its public credibility, promote disunity in its leadership, and weaken party morale and confidence enough to affect the timing of elections (Boston, 1980, p. 107; Kay, 1981, p. 37). More seriously, by-election reversals can even cost governments their majority status in Parliament, as happened to the last British Labour government (Butler and Kavanagh, 1981). Consequences like these are the inevitable risk attaching to the tendency for governments almost invariably to lose support in by-elections relative to the preceding general election. A British example will suffice to illustrate this point. Eighty-six percent of the almost 300 by-elections held between 1950 and 1983 registered a drop in the governing party's vote, the mean value of which over all contests is 9.7%. The picture with regard to seat losses is less startling, but governments still lost 42 of them and gained only 3 (see also Butler, 1973, p. 9; Boston, 1980, p. 107; and Scarrow, 1961).

In view of their ever-present potential for making life difficult for the party in power, it is understandable that the question most commonly asked about by-elections is why governments do so badly in them (Boston, 1980; Butler, 1949, 1973; Kay, 1981; King, 1968; Pollock, 1941; Scarrow, 1961; Sparks, 1940; Stray and Silver, 1982, 1983; Taylor and Payne, 1973). What is less understandable is the lack of any serious attempt to formulate and test empirically hypotheses addressing this question. Using Britain as a case study, this article goes some way toward remedying this neglect. Since attitudinal data from the constituencies in which by-elections have taken place are simply not available in the public domain, the analysis of government vote losses undertaken here is necessarily based on aggregate data alone. The undoubtedly important effects of individual perceptions on this aggregate-level phenomenon cannot be assessed until suitable data are available (as, for example, in Kernell, 1977).

The basic strategy of this analysis is to identify three separate political explanations of why governments do so badly in midterm elections: the *turnout*, *referendum*, and *personal vote* models. Following existing practice, these models will first be tested individually. It will then be argued, however, that this methodology represents an inadequate conceptualization of the individual and collective effect of these models, since it does not allow for the interplay between them. Integrating them into a single causal model, it will be shown, provides an account of their role in explaining governments' by-election performance that is more sensitive and sophisticated than any of the models affords individually.

By-Elections in Britain

Postwar Britain is perhaps the best available research site for an analysis of the dynamics of government support in by-elections, for three reasons. The first is that the combination of a large House of Commons and a long, five-year parliamentary term means that by-elections are far more numerous there than in, for example, the other Anglo-American parliamentary democracies, where the size of the lower house tends to be smaller and the parliaments of shorter duration (Inter-Parliamentary Union, 1976). In the years between 1919 and 1970, for example, there were 795 by-elections in Britain (Butler, 1973, p. 6), whereas only 94 took place in New Zealand over a period 10 years longer (Boston, 1980, p. 104). With 110 contests between 1901 and 1982, Australia is very similar to New Zealand, while Canada, with 152 contests in the years from 1940 to 1980, scores higher than both of them but still falls far short of Britain (Australian Electoral Office, 1983; Kay, 1981, p. 39).

Britain's second advantage is that its two major parties have spent more or less equal periods of time in office in the postwar period: 21 years for the Conservatives, and 17 for Labour. In consequence, swings against the government cannot be construed simply as swings against a party perceived as having dominated the national political process for too long. Finally, the political importance of by-elections in Britain has ranged from the slight to the serious. To be sure, most by-elections have represented no immediate threat to the incumbent government, but it is worth bearing in mind that 4 of the 11 governments elected since 1950 enjoyed small majorities. The 1950 government had a majority of five seats, the 1964 one a majority of four; the government of February 1974 was a minority government, and that of October the same year enjoyed a majority of only three seats at its election. The very fact that governments do as badly in by-elections under these conditions as when they have a large majority means that the forces shaping their performance should be particularly apparent in Britain.

This analysis begins with the 1950 general election because it was the first postwar election not characterized by multimember constituencies and plural voting; it ends with the 1983 general election because it is the most recent. A total of 296 by-elections were held in Britain over this period. Excluded from their number, however, are three that were not contested by the government in either the preceding general election or the by-election. This leaves the 293 constituencies that form the data base for this analysis, a full list of which can be found in Mughan (1986, appendix A).

Three Models of Government Vote Losses

Although never tested empirically, there has been informed speculation about why by-elections go against the party in power. This speculation can be distilled into three distinct models, the rationales for which follow.

The Turnout Model

This model is dealt with first because it apparently enjoys the status of conventional wisdom among observers of by-elections. Its focus is voting turnout, since, as with the governing party's vote, this is almost always lower in midterm than in general elections. In Britain, for example, this was the case in all but 15 of the relevant 293 by-election contests. With a range stretching from an increase of 11.4% to a decrease of 39.4%, the drop in turnout averaged 14.7%; it correlates with the change in the government's vote at −0.33. In other words, the bigger the drop in turnout, the larger the drop in the government's share of the vote.

It is commonplace to assume a causal link between these trends: people who previously voted for the government stay away from the polls in by-elections in disproportionate numbers. Their reason might be dissatisfaction with their party's performance in office, or that they were not strong supporters of the government in the first place and simply cannot be bothered to turn out for an election where the encouragement to vote is weak and the political stakes low (Boston, 1980, pp. 117–22; Campbell, 1966; King, 1968). Whatever its cause, the explanatory importance of the decline in turnout seems self-evident to many, journalists as well as academics; for example, "The feature of the Walthamstow and Brighton by-elections was the abnormally low polls, mainly to be explained by the massive abstentions of Labour voters who have lost faith and hope in the [Labour] Government" (Wood, 1969, p. 1).

Before the turnout model can be adequately specified, however, it must be recognized that the outcome of any by-election will not be a function solely of change in the value of a number of variables, like turnout, from the preceding general election. Some contribution to the changing pattern of party competition will also likely be made by the cross-sectional value of these same variables at the time of the by-election. One hypothesis, for example, is that high turnout in a by-election will favor the government because it means that "the swing against [it] will be damped down" (King, 1968, p. 415). Thus, both the cross-sectional and change versions of pertinent variables must be included in the specified model if a reliable estimate of their independent effect on the dependent variable is to be obtained. The turnout model, then, is

$$\Delta GPV = a + b_1 T + b_2 \Delta T + e_i, \qquad (1)$$

where ΔGPV is the governing party's percentage of the poll in the preceding general election minus its share in the by-election; T is the percentage of by-election turnout; ΔT is the percentage of turnout in the preceding general election minus the same figure in the by-election; and e_i represents the stochastic error. The expectation is that the partial regression coefficient, b_1, will significantly reduce government vote losses, and b_2 significantly increase them.

The Referendum Model

The second model has its origins in the long-standing and unresolved debate over whether by-election outcomes in single constituencies can be taken as a signal of anything about the nation's assessment of the incumbent government's performance in office. In Britain, this debate was at its height in the throes of the country's democratization, when electoral patterns were still being set. In 1904, for example, Prime Minister Arthur Balfour roundly rejected the view that by-elections are referendums on the national government:

> I do not for one instant admit that by-elections are a test, or ought to be regarded as a test, of public feeling. They are, of course, a test of the feelings of a particular constituency at the time the by-election takes place. They are not, and

> they cannot be made, the index and test of what the feeling of the people of the country is as a whole. (quoted in Pollock, 1941, p. 528)

Subsequent empirical studies added weight to this verdict (Pollock, 1941; Sparks, 1940).

Evidence is always questionable, however, and Balfour's interpretation was unanimously accepted neither then nor in more recent times. Assessing the same interwar contests, Herman Finer, for example, confidently declared by-elections to function as "invaluable political referenda on the action of the government" (quoted in Pollock, 1941, p. 519). This interpretation is based on two assumptions that seem hardly contentious in the contemporary British context. The first is that governments will decline in popularity simply by taking and implementing decisions that are not always acceptable to all elements of the support coalition that brought them to office in the first place. The second is that because the British electorate is relatively unmoved by local forces (Stokes, 1967), a government's decline in popularity will mmanifest itself in by-election outcomes, since these will accurately reflect trends in national public opinion; nor is this an interpretation to be taken lightly. The imputed role of individual by-elections as a referendum on the government's performance is the essential reason for the considerable media coverage of them, even when their outcome does not threaten the distribution of parliamentary power. It is also one reason why governments dislike doing badly in them, even if their public posture is to shrug off poor performances as routine, midterm reversals.

But before the referendum model can be tested, it must be noted that a full specification of this model must take into account that governments do not operate in a political vacuum; there are always other parties waiting in the wings to take up the reins of office. In an adversarial polity like Britain, the government's loss of popularity might automatically be thought to be the opposition's gain. If this were the case, there would be no need to ascertain whether changes in the opposition party's national standing affect government vote losses, since the relationship would be the exact inverse of the governing party's. Things are not so simple, however, since support for the government or support for the opposition are not the only alternatives open to electors, even in perfect two-party systems. Another is for electors not to express a voting preference at all. In addition to the matching measures for the governing party, therefore, a complete specification of the referendum model dictates the incorporation of cross-sectional and change measures of support for the opposition party and of the proportion of the electorate not knowing which party to support.[2] The model thus takes the following form:

$$\Delta GPV = a + b_3GA + b_4\Delta GA + b_5OVI + b_6\Delta OVI + b_7DK + b_8\Delta DK + e_i, \qquad (2)$$

where *GA* is the proportion of Gallup poll respondents expressing approval of the government's record in the month of the by-election; ΔGA is the change in this proportionate figure from general to by-election; *OVI* is the proportion of respondents intending to vote for the major opposition party; ΔOVI is the change in this figure between the two types of election; *DK* is the proportion volunteering a "don't know" voting intention; and ΔDK is the change in this proportion.[3]

It is expected that the more popular the government is at by-election time (b_3), the lower will be its vote losses; conversely, the more popular the opposition (b_5) is, the higher will be these losses. As for the change measures on these two variables, the direction of their impact will depend

Table 1. Variables, Scoring, and Means

Variables	Scoring	Means
Control Variables		
Partisanship	10.9% to 21.0%	18.07
Δ Partisanship	6.7% to -4.9%	0.34
Country	1 = Scotland/Wales, 0 = England	0.18
Season	1 = Summer/Spring, 0 = Winter/Autumn	0.43
Register age	0.5 to 16 months	9.45
Δ Register age	-12.0 to 10.5 months	-0.30
Electoral marginality	0.20% to 73.0%	22.34
Δ Electoral marginality	58.3% to -36.3%	1.81
Parliamentary marginality	-33 to 102 seats	46.85
Δ Parliamentary marginality	30 to -2 seats	4.21
Number of by-elections	1 to 6	1.90
Timing of by-elections	1 = within 6 months of a general election, 0 = not within 6 months	0.18
Number of candidates	2 to 16	3.79
Δ Candidate number	3 to -11	-1.10
Turnout Model		
Turnout	24.9% to 85.4%	61.21
Δ Turnout	39.4% to -11.4%	14.71
Personal Vote Model		
Δ Constituency service	40.3 to 0 years	7.89
Δ Privy Council membership	25.8 to 0 years	3.67
Referendum Model		
Government approval	18.0% to 60.0%	39.27
Δ Government approval	35.0% to -22.0%	6.65
Opposition voting intention	25.0% to 55.0%	45.60
Δ Opposition voting intention	16.0% to -19.0%	-1.72
Don't know voting intention	6.0% to 25.0%	12.35
Δ Don't know voting intention	8.0% to -12.0%	-1.89

Note: The symbol Δ denotes change, and the change measures are arrived at by subtracting the by-election score from the preceding general election score.

on the direction of their change. Table 1 shows governments to become on average more unpopular between general and by-elections (b_4), and the larger the drop in their support, the larger should be their vote losses. The opposition, in contrast, gains support, so that the change in its popularity (b_6) can also be expected to be positively related to the dependent variable. Finally, on the assumption that governments are the principal midterm reference points for voters, the higher the proportion of voters volunteering a "don't know" response (b_7) and the larger the increase in this figure (b_8), the greater should be the drop in the government's share of the vote.

The Personal Vote Model

The final model to be tested shifts the focus of attention from national party politics to what might loosely be called personality politics. By-elections are caused by the death or resignation of individuals who have been the member of Parliament (*M.P.*) for their constituency for some years. In the particular context of Britain in the period from 1950 to 1983, the length of such service for departing government M.P.s ranges from less than 1 year to over 40 years, and averages a little more than 10 years. The point of this observation is that evidence is now accumulating that M.P.s are able to build up a "personal vote" as a direct result of the public visibility and opportunities for constituency service that tenure of office gives them (Cain, Ferejohn, and Fiorina, 1984; Williams, 1966-67). One advantage of a personal following is that it provides the individual M.P. with some insulation against the worst effects of downturns in the popularity of his party. His departure from office, however, should mean the loss of this insulation for the by-election candidate nominated to replace him, and lead to an intensification of the vote swing against the government.

Nonpartisan service to both constituents and constituency is the commonly recognized basis of the personal vote (Cain et al., 1984, p. 113). But this unidimensional conception is incomplete in parliamentary systems in which the executive and legislative branches of government are fused. This is because parliamentarians can be members of both branches, and may consequently develop a personal following through their service to the nation instead of, or as well as, to their constituency. The most salient badge of national political prominence is the title *Right Honorable* that membership in the Privy Council brings, and there is no reason why the visibility and status accruing to those so honored should not be translated into a nonpartisan support base among constituents proud of their representative's achievements. This being the case, the longer the departing government M.P. has enjoyed this prominence, the greater should be the vote swing against his party in the by-election.[4]

No governing party candidate had previously represented the by-election constituency in the Commons, which means that none of them had had the opportunity to build up a personal following there. The cross-sectional personal vote variables are therefore redundant, and the model to be tested is

$$\Delta GPV = a + b_9 \Delta CS + b_{10} \Delta PC + e_i, \qquad (3)$$

where ΔCS is the number of years the outgoing governing party member had served as parliamentary representative of the constituency, and ΔPC is the number of years the same individual had been a member of the Privy Council. Both variables are expected to be positively related to the size of the governing party's vote losses.

Testing Models of Government Vote Losses

These models, then, represent common explanations of the by-election performance of governments in the short term. Voting patterns in both general and by-elections, however, are also prey to two other sets of forces whose influence has to be controlled if the turnout, referendum, and personal vote models are to be tested properly. These are, first, long-term forces whose effect is more or less stable across elections, and, second, contextual forces specific to the by-election contest.

When taken together, these long-term and contextual forces are numerous, and reasons of space dictate that the theoretical grounds for their inclusion as control variables not be spelled out in detail here. This detail is given in Mughan (1986) and a summary identification of their measurement and values can be found in Table 1. To start with the long-term electoral forces, the first of them is aggregate partisanship, by which is meant the degree to which individuals' party preferences can be predicted from a knowledge of their sociodemographic characteristics. Following Powell (1980), the degree of partisanship in the electorate is measured through election-by-election estimates of the variance explained in party choice by an unchanging set of social structural variables (Rose and McAllister, 1986, p. 92).[5] Then comes the country—England on the one hand, Scotland and Wales on the other—in which the by-election is held (Mughan, 1986). The season in which elections take place is also often held to influence voting patterns (Alderman and Cross, 1975, p. 388; King, 1968, p. 415).

The contextual variables are more numerous. The first of them is marginality, and it takes two forms: electoral and parliamentary. Electoral marginality is best known for its positive effect on turnout (Denver and Hands, 1974), but it also needs to be controlled in case the swing against the government is, other things being equal, less marked in more marginal constituencies. As for parliamentary marginality, it would seem reasonable to hypothesize that the smaller the government's overall majority in Parliament, the greater will be its efforts to dampen, or even reverse, its vote losses in by-elections. The timing of by-elections has a threefold potential to affect their outcome. In the first place, the larger the number of by-elections scheduled on the same day, the higher their stimulus and turnout levels are likely to be, and the lower the vote swing against the government. Second, the closer they are held to a general election, the lower the government's vote losses (Taylor and Payne, 1973, pp. 345–49). Third, if turnout does indeed affect the size of the government's by-election vote, then the age of the electoral register should be controlled, since deaths, removals, and so on mean that turnout gets lower as the register ages. Finally, account must be taken that minor parties, especially the Liberals, have shown themselves more likely to contest by-elections than general elections. The larger number of candidates in the former, therefore, is likely to accentuate government's vote losses by providing an alternative for voters who might otherwise have supported the government.

This array of electoral forces will act as control variables when each of the models —turnout, referendum, and personal vote —is tested for its ability to explain governments' by-election vote losses. The analysis relies on ordinary least squares regression methods, which assume that variables are, to a reasonable approximation, linear and additive (Hanushek and Jackson, 1977).[6] Missing data, being very rarely a problem, are treated by pairwise deletion. Since a directional effect has been hypothesized for each variable in the models, one-tailed tests of significance will be reported.

Table 2 presents the regression results for each model.[7] The immediately striking feature of the table is its clear indication that only the referendum model has an unambiguous role to play in the explanation of governmental performance in by-elections. The other two models produce disappointing results at best. The worst performance comes from the personal vote model, in the sense that the table provides no evidence that the loss of a long-serving elected representative has any direct effect on the pattern of constituency vote losses for governments. Moreover, this observation holds whether or not this representative also enjoyed national political prominence. Thus, the evidence would seem to vindicate the traditional supposition in British politics that "electors vote for parties and not for candidates" (Jennings, 1961, pp. 260–61). It may be, of course, that this disregard for candidates is less pronounced in the contemporary era of partisan dealignment, but in the longer term it would appear that any personal following that departing governing party candidates may have had is not sufficiently strong to have made itself felt over the generalized anti-government swing characteristic of by-elections.

The explanatory status of the turnout model is a little more ambiguous, since the effect of change in turnout on the dependent variable is significant at the .05 level only. The relationship is certainly not of the strength that would have been predicted, in view of the relative ease with which relationships of this level of significance are achieved with aggregate

Table 2. The Effects of the Turnout, Personal Vote, and Referendum Models on Governments' By-Election Vote Losses

	Turnout Model			Personal Vote Model			Referendum Model		
Variables	*b*	beta	*t* statistic	*b*	beta	*t* statistic	*b*	beta	*t* statistic
Control									
Partisanship	-0.36	-0.10	1.69*	-0.34	-0.10	1.60	0.55	0.16	1.72*
Δ Partisanship	0.13	0.04	0.64	0.16	0.05	0.80	0.23	0.07	0.85
Country	0.76	0.03	0.74	1.21	0.05	1.21	2.59	0.11	2.85**
Season	2.05	0.12	2.39**	2.20	0.13	2.55**	0.45	0.03	0.55
Δ Register age	-0.12	-0.06	1.24	-0.10	-0.05	1.00	0.05	0.02	0.50
Electoral marginality	-0.02	-0.03	0.50	0.01	0.02	0.31	0.00	0.00	0.00
Δ Electoral marginality	0.10	0.18	3.26**	0.09	0.16	2.87**	0.11	0.19	3.86**
Parliamentary marginality	0.10	0.43	7.38***	0.11	0.44	7.44***	0.08	0.35	4.74***
Δ Parliamentary marginality	0.12	0.10	1.54	0.13	0.10	1.58	0.10	0.08	1.28
Number of by-elections	0.05	0.01	0.13	0.00	0.00	0.00	0.27	0.04	0.88
Timing of by-elections	-2.35	-0.10	2.34**	-2.17	-0.10	2.16*	-1.19	-0.05	1.26
Δ Candidate number	1.66	0.31	6.65***	1.71	0.32	6.88***	1.38	0.26	5.73***
Turnout									
Δ Turnout	-0.10	-0.11	1.96*	—	—	—	—	—	—
Referendum									
Government approval	—	—	—	—	—	—	-0.58	-0.66	7.26***
Δ Government approval	—	—	—	—	—	—	0.16	0.24	2.08*
Δ Opposition voting intention	—	—	—	—	—	—	0.01	0.01	0.09
Don't know voting intention	—	—	—	—	—	—	0.36	0.14	1.97*
Δ Don't know voting intention	—	—	—	—	—	—	0.25	0.10	1.42
Personal Vote									
Δ Constituency service	—	—	—	0.07	0.08	1.47	—	—	—
Δ Privy Council membership	—	—	—	-0.03	-0.02	0.32	—	—	—
Intercept	16.4			11.5			18.8		
Adjusted R^2	46.4			45.9			59.1		

Note: The symbol Δ denotes change. Dashes indicate variables not figuring in the model.

*$p \leqslant .05$ (one-tailed test).

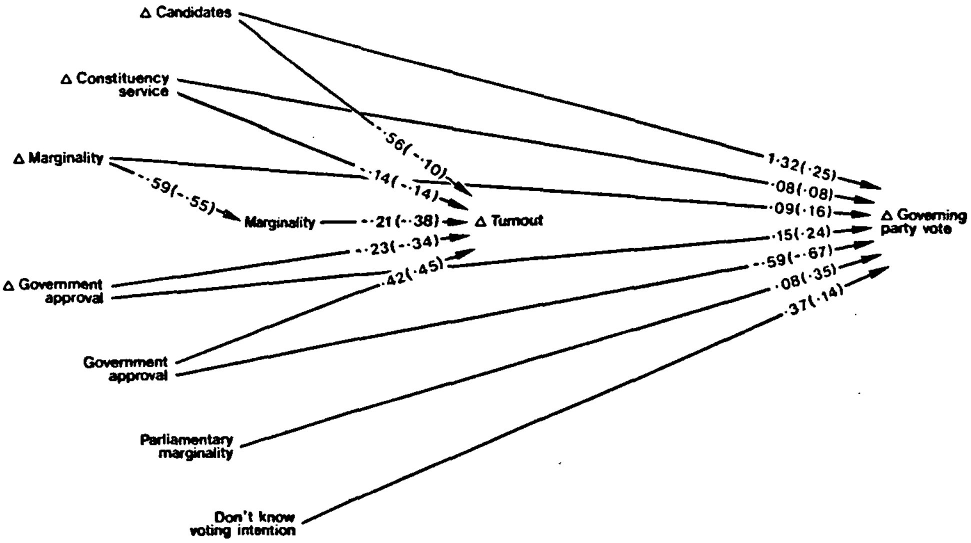
**$p \leqslant .01$ (one-tailed test).

***$p \leqslant .001$ (one-tailed test).

data, and of turnout's bivariate correlation of −0.33 with change in the governing party's vote, and of the prominent explanatory role commonly attributed to it by observers of by-elections. Under these circumstances, the question inevitably arises as to whether it can be considered a "real" relationship.

Two features of Table 2 suggest that the best way out of this interpretational dilemma is to reconceptualize the basic relationship rather than to opt for some statistical determinant of substantive importance that is itself somewhat arbitrary anyway: First is the fact that a number of background variables significantly moderate the relationship of turnout to vote, which suggests that the relationship itself may be spurious; second is the impact of the cross-sectional and change versions of the government approval variable. If these aspects of the government's standing in the polls have a substantial impact on the change in its vote, there is no immediately apparent reason why they should not have a similar influence on turnout change. After all, if a government is unpopular nationally, its supporters are at least as likely to "vote with their feet" and abstain as they are to vote for another party. Again, therefore, the relationship of turnout to vote could

Figure 1. A Causal Model of Governing Parties' By-Election Vote Losses


Δ Candidates
Δ Constituency service
Δ Marginality
Δ Government approval
Government approval
Parliamentary marginality
Don't know voting intention
Marginality
Δ Turnout
Δ Governing party vote
-·56(-·10)
-·14(-·14)
-·59(-·55)
-·21(-·38)
-·23(-·34)
.42(·45)
1·32(·25)
·08(·08)
·09(·16)
·15(·24)
-·59(-·67)
·08(·35)
·37(·14)

Note: The model shows metric partial regression coefficients (*b*s) and standardized regression coefficients (betas) in parentheses. Only paths at $p \leqslant .05$ (one-tailed test) are shown. The model also controls for all the variables shown in Table 2 and not figuring herein. See Table 1 for the definition of the variables.

be spurious, and the basic model needs to be respecified to take account of this possibility.

Figure 1 depicts a path model of government by-election vote losses. Its advantage over the previous treatment of the three models separately is that it allows turnout change to function as both an independent and a dependent variable. That is, it admits of the possibility that while turnout change may influence vote losses, it in turn may be influenced by the government approval and personal vote models, with the result that the direct relationship between turnout and the vote becomes spurious. In testing this possibility, a number of steps have been taken to simplify the model in Figure 1. First, the long-term forces of partisanship, country, and season were included in the estimation of the model, but are excluded from its presentation. Second, the model presents only the election-specific variables that either directly influence the change in the government's vote or influence the change in both turnout and the vote. Variables affecting turnout change alone are excluded, because their failure to influence the vote loss variable either directly or indirectly means they are irrelevant to the primary concern of this analysis. Finally, even though the change variables will inevitably influence some of the cross-sectional ones, both are treated as background variables occupying the same position in the causal sequence of the model as a whole. Their interrelationship is irrelevant to the question of change in the governing party's vote.

Bearing in mind the previous discussion of the separate models, the main feature of Figure 1 is its striking confirmation that the bivariate relationship between inter-election change in turnout and in the governing party's vote is indeed spurious. Once variables comprising the government approval and personal vote models are conceptualized as "causing" turnout change, this relationship disappears. Its nonexistence is clearly illustrated in its partial regression coefficient of −0.01, its standard error of 0.05, and its *t* statistic of 0.21. The four variables responsible for the spurious relationship are government approval, change in the level of government approval, change in the number of candidates, and change in the length of the governing party candidate's constituency service. All four are related in one direction to turnout change and in the other to change in the government's vote share. The effect is to produce the original, negatively signed bivariate relationship.

Further inspection of Figure 1 shows that the path model is heretical not only for its denial of the commonly assumed importance of the turnout model, but also for its affirmation of the commonly dismissed importance of the personal vote model, or at least its constituency service aspect. The effect of controlling for the nation's assessment of the governing party's record in office is to allow the personal following of its departing M.P.s to surface as a significant factor in the explanation of their former party's by-election misfortunes. As would be expected in a party-dominated British electorate, of course, the effect of this variable is not strong; measured by length of constituency service, it has in fact the weakest impact of all the predictors in the model on the vote change variable, and the second weakest impact on turnout change. Nonetheless, its effect is undeniable.

The limited contribution of the departure of its sitting M.P. to the governing party's by-election misfortunes falls into perspective when compared with the structuring effect of the referendum model. In terms of its expected explanatory importance, this model enjoys mixed fortunes. On the debit side, there is the absence of the hypothesized stimulant effect for opposition party support. On the credit side, there is the overwhelming preeminence of the cross-sectional government approval variable; its (depressant) effect on government vote losses is almost twice as great as that of any other single predictor, and more than eight times as great as that of the constituency service variable. Each of these themes will now be elaborated.

A common characterization of the British political process is that it is an adversarial one with government and opposition preying on each other's misfortunes, and locked into a continuous struggle to convince the electorate of the superiority of the claim of each to the right to govern. With a disciplined opposition always sniping at and harassing an equally disciplined government, this dialectic obviously structures the British parliamentary process, but Figure 1 indicates that it exercises no comparable effect on the behavior of the electorate. Be it measured in change or cross-sectional terms, voting intention for the major party in opposition (or, indeed, for the Liberal party separately) simply does not affect the governing party's by-election performance.

The reason for its irrelevance at the electoral level is that the major party in opposition shares with the Liberal party in particular the role of beneficiary of popular discontent with the government. Indeed, it finds itself subordinated to the Liberal party in this role, as is evident from the correlation of government voting intention with opposition voting intention at −0.29, and with Liberal voting intention at −0.48. The corresponding correlations for the change measures on these variables are 0.33 and 0.43 respectively. It is only when voting intention for the two parties is aggregated to form a single measure of opposition popularity that such an impact manifests itself in a reestimated version of the path model in Figure 1.[8]

The fragmentation of the opposition at the electoral level leads logically to the most salient feature of Figure 1, which is its demonstration that governments, to the extent they are able to shape the public's evaluation of their record in office, are in considerable control of their own fate in by-elections. By far the most potent predictor of government performance is precisely the proportion of the electorate satisfied with this record in the month of the by-election. This satisfaction may be based on the country's macroeconomic performance, the popularity of the Prime Minister, or any number of factors; the important point is the higher it is, the lower the governing party's vote losses in the by-election.[9] Moreover, the less that the proportion satisfied has dropped relative to the figure prevailing when it took office, the lower its vote losses as well.

These relationships are all the more robust because the model has taken account of the governing party's standing in the House of Commons. The strong effect for the parliamentary marginality variable in Figure 1 shows that the by-election vote swing is more pronounced against governments with large majorities, regardless of how popular or unpopular they may be with the electorate. This suggests that disgruntled supporters are less reluctant to desert their party when it has a safe majority in Parliament, since desertion entails no risk of its losing office. When, in contrast, government majorities are small, supporters are more likely to remain loyal in by-elections, so as not to jeopardize their party's control of government. This observation does not, however, gainsay the overriding explanatory importance of the public's satisfaction with the government's performance in office. The simple fact of the matter is that government vote losses in by-elections are primarily a function of the pattern of such satisfaction at the time the by-election is held. The parliamentary situation may mediate this relationship, but it does not overshadow it.

Conclusion

In highlighting the reasons for government vote losses in by-elections relative to the preceding general election—a matter at best imperfectly understood in the past—this analysis has served to illustrate sharply the need to put conventional wisdom to the empirical test. Turnout change, the factor that has been commonly accepted by academics, journalists, and party activists alike to be responsible in large part for these losses, has been shown in fact to be no more than spuriously related to them. Moreover, even at the bivariate level, the relationship between turnout change and vote change runs counter to that posited by proponents of its importance. Ironically, what turns out to have greater value is the erstwhile incumbent's personal vote, based on constituency service—a factor that has probably been as widely dismissed as turnout change has been embraced. The longer the departing government M.P. has represented the constituency, the worse will be his party's midterm performance.

Pride of explanatory place, however, goes to the variable whose actual role in bringing about a drop in the government's by-election vote has been subject to the most disagreement and liveliest debate, namely, the government's national standing. Moreover, by far the stronger effect is exercised by the level of such standing in the month of the by-election, and not by the extent of the change in it since the last general election. In other words, what determines how well governments do in by-elections is less the change in their popularity from the time of their election than how high they stand in national public opinion at the time of the by-election. It follows that by-elections can quite legitimately be interpreted as national referendums on government performance. Indeed, what emphasizes this aspect of their character is the fact that national support for the major opposition party or for the Liberal party separately plays no role in determining the size of the swing against the government. Insofar as the electorate reacts principally to the current standing of the government and disregards stasis and change in the popularity of the other major parties offering themselves as a potential alternative government, by-election voting patterns would seem to represent an investment in the present rather than in the future.

A word of caution is nevertheless in order. It must be stressed that the role of by-elections as barometers of national public opinion about the government does not mean that their outcome can be taken as a faithful indicator of how that government would do in a general election, even if it were held the same day. This is because approval of its record in office will not always mean a vote for the government, and disapproval a vote against it. In both cases, a critical threshold will have to be crossed before government supporters desert their party or opposition supporters defect to it (Hudson, 1985; Kernell and Hibbs, 1981). It also stands to reason that if by-election outcomes are poor predictors of proximate general elections, they will be even worse predictors of more distant ones. Thus, compounding the problem of the discrepancy between approval and vote is the impossibility of predicting how popular governments will be when the next general election is called. The normal pattern is for satisfaction to be high when governments are first elected, for a dip in midterm and for a recovery in the run-up to the next general election. But there remains considerable variability within this broad pattern. Some governments (e.g., the 1966–70 Labour one) end up being less popular than at the outset despite this recovery, whereas others (e.g., the 1979–83 Conservative one) finish up being more popular. In short, by-elections may be useful barometers of the nation's evaluation of the incumbent government at the time they are fought, but they ought not to be used to predict general election outcomes in the near or distant future.

Notes

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1. The inappropriateness of these comparisons emphasizes the point that the true U.S. equivalent to the parliamentary by-election is not the off-year congressional election, but the "special election" to the House of Representatives. As with by-elections, however, this type of election remains *terra incognita* (Sigelman, 1981).

2. Another option, of course, is to express an intention to vote for a minor party. In Britain, the only such party for which polling data are available is the Liberal party; its role in the explanation of government vote losses in by-elections will be dealt with later in this article.

3. The Gallup poll questions and monthly figures are taken from Webb and Wybrow (1981, 1982). The 1982 and 1983 figures were given to me in private correspondence by Bob Wybrow of the Gallup Organization.

4. The number of years served as a member of Parliament or of the Privy Council is not an altogether satisfactory measure of the personal standing of M.P.s., since it does not allow for differences in such individual characteristics as vivacity, assiduity in "nursing" the constituency, and general devotion to the job of being an M.P. Nor does it allow for differences in the public prominence of the positions of responsibility held by members of the Privy Council. It might be pointed out, however, that the very blandness of the measures used means that the personal following of the genuinely popular M.P. will be underestimated.

5. Since Rose and McAllister (1986, p. 92) provide partisanship figures for 1959 to 1983 only, the equivalent figures for the general elections in 1950, 1951, and 1955 are necessarily estimates. These were derived by equating Rose and McAllister's 1959 figures with Alford's (1963, p. 103) index of class voting value for that same year. The result of dividing the former by the latter is a figure of 0.53%. In

other words, one point on Alford's index for 1959 was the equivalent of 0.53% of the variance explained by social structure in the same year. Assuming a stable relationship between the two measures over the course of the 1950s, Alford's 1950, 1951, and 1955 index values were then multiplied by 0.53 to give the general election partisanship values for these years. Their by-election counterparts were taken to be a linear function of the change in partisanship's value between the general elections immediately preceding and succeeding a by-election. Thus, if partisanship increases by a value of, say, 2% between two general elections, and a general election is held halfway between them, then the by-election partisanship score will be the variable's value at the preceding general election plus 1%.

6. In the exploratory data analysis, a turnout change quadratic term was included in the regression equation to determine whether there was a significant element of curvilinearity in this variable's relationship to the dependent variable. There proved to be none at all, so the quadratic term was not used subsequently.

7. Missing from Table 2 are the cross-sectional versions of the register age, candidate number, turnout, and opposition voting intention variables. These were excluded from the analysis because each is correlated at an unacceptable level (−0.80, −0.92, −0.78, and −0.80, respectively) with its corresponding change measure (Hanushek and Jackson, 1977, pp. 87–88). To avoid the unstable estimates that usually result from such high levels of collinearity, the cross-sectional measures were excluded from further consideration, on the grounds that change, and especially change in turnout, plays a more central explanatory role in the models to be tested. It might be noted that including the cross-sectional measures instead hardly alters the pattern of influences on government vote losses depicted in Figure 1. Details are available from the author on request.

8. The path model had to be reestimated to ascertain the effect of the aggregated opposition variable, because the Liberal party contested only 179 constituencies in both the general and the by-election. The reestimated model is not presented in the text partly because it departs little from Figure 1, and partly for reasons of space. Details are available from the author on request.

9. In the exploratory analyses, lags of up to three months on the independent voting intention and government approval variables were tried. The best predictions, however, came from the scores on these variables in the month of the by-election.

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TRAPPING THE PRINCE: MACHIAVELLI AND THE POLITICS OF DECEPTION

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Machiavelli's most famous political work, The Prince, *was a masterful act of political deception. I argue that Machiavelli's intention was a republican one: to undo Lorenzo de Medici by giving him advice that would jeopardize his power, hasten his overthrow, and allow for the resurgence of the Florentine republic. This interpretation returns* The Prince *to its specific historical context. It considers Machiavelli's advice to Lorenzo on where to reside, how to behave, and whom to arm in light of the political reality of sixteenth-century Florence. Evidence external to* The Prince, *including Machiavelli's other writings and his own political biography, confirms his anti-Medicean sentiments, his republican convictions, and his proclivity for deception. Understanding* The Prince *as an act of political deception continues a tradition of reading Machiavelli as a radical republican. Moreover, it overcomes the difficulties of previous republican interpretations, and provides new insight into the strategic perspective and Renaissance artistry Machiavelli employed as a theoretician.*

> [F]or some time I have never said what I believed and never believed what I said, and if I do sometimes happen to say what I think, I always hide it among so many lies that it is hard to recover.
>
> —Machiavelli

Realism is generally considered a necessary first move in the effort to make the study of politics scientific, and when political scientists turn to the history of ideas, they tend to acknowledge Machiavelli as the champion of realism, and *The Prince,* in particular, as the first treatise in political thought to infuse the contemplation of political affairs with a spirit of empiricism, *realpolitik,* and *raison d'état.* By now it has become commonplace for political scientists in fields as diverse as international relations, comparative politics, organization theory, and political psychology to construct explanations about political life and political conduct that rely in part on the "axioms" that evolved from Machiavelli's little treatise. These include the necessities of "naked self-interest," the maintenance of rulership at all costs, the utility of unethical and manipulative behavior, and the centrality of power as an end in and of itself.[1] Accordingly, in many areas of political inquiry Machiavelli has come to be regarded as the theorist of "Machiavellianism," and Machiavellianism itself en-

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tails understanding politics primarily in terms of who dominates whom and how successfully.

As far as it goes, this understanding of politics as power is true to Machiavelli's purposes in *The Prince*, but, in the end, I think it does not go far enough. What is missing from the political scientists' rendition of Machiavelli is another vision of politics, a republican one rooted in love of liberty and respect for self-governance, which political theorists have long considered as vital to Machiavelli's thought as power and *realpolitik*.[2] This second vision of politics as participation is one theorists tend to associate with Machiavelli's later works, particularly *The Discourses* and *The History of Florence*. Thus, like political scientists, they too often read *The Prince* in terms of the politics of power and domination.

I suggest that neither political scientists nor political theorists have realized the full force of the Florentine's intentions in *The Prince*. By offering a different perspective on the treatise I hope to show that both groups have underestimated Machiavelli, and in ironic ways—the theorists by not seeing the republican goals that guide the treatise, the political scientists by not recognizing the full force of the "Machiavellian" intentions that inform it. My purpose, then, is not only to present a new interpretation of this most famous of Machiavelli's works, but also to encourage a reconsideration of the adjective that bears his name and the vision of politics it represents.

Republicans and *The Prince*

No political thinker was more aware of how crafty assault by deceit could serve as a substitute for brute assault by violence than Niccolò Machiavelli. The theme of deception weaves through all of his work—his drama, his military writings, his history, his political theory. *Mandragola* is a tale of crafty assault practiced by the wily Ligurio, who helps a young rake bed the beautiful wife of a pompous old doctor. *The Art of War* argues that a commander who vanquishes an enemy by stratagem is as praiseworthy as one who gains victory by force. *The History of Florence* tells the story of a city where deceit and guile secure power, while honesty and blind trust ruin it. Nowhere, of course, is Machiavelli's love for the art of deception more vividly unmasked than in *The Prince*. There the subject of crafty assault takes its notorious form in his advice to a ruler on how to play the fox, "confuse men's brains," and employ cunning in the political world. In short, whether the subject is love, war, or politics, Machiavelli recognizes the advantages of crafty assault in any form, be it trickery, stratagem, or artifice.

For those who believe that Machiavelli was a republican and a Florentine patriot, this view of him as the master theorist of deceit poses difficulties. As Hanna Pitkin (1984) reminds us, foxes make poor citizens—their deceit undermines civic *virtù*—and *The Prince* abounds with foxes and advice on deception. Furthermore, in Machiavelli's infamous tract to Lorenzo de Medici we find no defense of the Florentine republic, no call for popular liberty, no praises of republican Rome. Far from denouncing tyranny, as would any bold republican, Machiavelli appears to content himself with forging the absolute and ruthless power of an autocrat. How, then, is it possible to hail him as a defender of liberty, self-government, and civic *virtù*, when these appear to be the very values he teaches his Medici protégé to subvert?

Proponents of the thesis that Machiavelli was a republican, despite his authorship of *The Prince*, fall into two camps. According to the "weak republican" thesis, *The Prince* is an aberration. Despairing of the future of Florence, much less its republican government, Machiavelli saw the Medici as the only alternative to total chaos, and so wrote his advice book in reaction to an impending crisis (Baron, 1961; Hale, 1961; Pocock, 1975). This thesis maintains that after 1513 Machiavelli simply abandoned the idea of the prince as a "political innovator"; he renewed his commitment to republicanism, developed an admiration for antiquity, and refueled his antipathy for the Medici. The result of this renewal was *The Discourses*, complete with its dedication to republican sympathizers and its repudiation of the entire genre of princely advice books. Thus, these scholars view *The Prince* as a tract that reflects both Machiavelli's acceptance (reluctant though it might have been at the time) of Medicean domination and his clear-sighted, if opportunistic, attempt to ingratiate himself with the new rulers of Florence.

Among other things, the weak republican thesis strains credulity. To read *The Prince* as a sudden capitulation to Medici rule, or as a tool to curry favor is, arguably, to underestimate Machiavelli as a citizen and a theorist. Though I find Garrett Mattingly's (1958) ultimate assessment of *The Prince* unconvincing (for reasons I will explain shortly), I think he poses in dramatic terms the correct riposte to the weak republican thesis:

> I suppose it is possible to imagine that a man who has seen his country enslaved, his life's work wrecked and his own career with it, and has, for good measure, been tortured within an inch of his life, should thereupon go home and write a book intended to teach his enemies the proper way to maintain themselves. . . . But it is a little difficult for the ordinary mind to encompass. (1958, p. 485)

The other camp advances a "strong republican" thesis, arguing that even as Machiavelli writes *The Prince*, he remains a defender of republican liberty and an opponent of the Medici (Gentili, 1924; Mattingly, 1958; Rousseau, 1978; Spinoza, 1945; Wolin, 1960). However, if this is indeed the case, and if republican sympathies abound in *The Prince*, then something else must be moving beneath the surface of the text; some drama that the protagonist-prince does not see must be taking shape. This is precisely what the strong republican camp wants to argue, but as is so often the case with Machiavelli, there are differences of opinion on the matter of the subtext of *The Prince*. Three main views emerge.

The first is Rousseau's (1978, p. 88) claim that *The Prince* is a book for republicans. Rousseau argues that the advice book was not intended for the Medici at all, but rather to expose to the people the brutal ruthlessness of princes and lay bare their methods and madness. There is a paradoxical quality to this interpretation of the sort we have come to expect from Rousseau, namely, that even as Machiavelli is fashioning masks for a prince, he is unmasking him. By exposing the prince's stock-in-trade, Machiavelli is arming republicans with all the knowledge they need to avoid being deceived. A simple and telling criticism can, however, be lodged against this Rousseauian opinion: Machiavelli could not be writing a book for republicans, because he never intended that they read it. Interpretative accuracy often hangs on matters of practical political action, not to mention the intention of where to publish and for whom, and in this case we find no evidence that Machiavelli did or attempted to do anything with his treatise but send it to Francesco Vettori, his contact in the Medici Palace.

A second version of Machiavelli as a strong republican hardly takes *The Prince* seriously, for it assumes that the treatise was not intended seriously. Garrett Mattingly (1958) holds that Machiavelli's advice book is nothing more than a joke, a "diabolical burlesque" of the mirror-of-princes literature prevalent in the Renaissance. Nominally agreeing with Rousseau, Mattingly argues that Machiavelli wrote *The Prince* as an alarm, a "tocsin" to the people of Florence. However, this agreement is surely more literary than political, for, unlike Rousseau, Mattingly does not read the treatise as revealing certain truths about princely power that republicans should know. He sees *The Prince* primarily as a fine example of Machiavelli's dramatic temperament, a reflection of his ability simultaneously to shock and amuse his audience. Machiavelli surely intended to shock and perhaps to amuse, and *Mandragola* fully displays his dramatic skills, but this does not suffice to make *The Prince* a burlesque, even if a diabolical one. The most obvious problem with Mattingly's reading is that it fails to take seriously Machiavelli's desire to reconstitute the political world. In *The Prince*, Machiavelli (*P*, p. 56) declares his intention "to write something of use to those who understand."[3] He wishes to *reveal* reality, not ridicule it, and his repeated instruction concerning princely *virtù* sounds less like a satire on the corruption of power than an attempt to determine how those in power might use guile and deceit to mitigate the corruption of the state.

The third strong republican perspective reads *The Prince* literally, as an advice book for a founder. Machiavelli's prince is to be the restorer of order, the man of *virtù* who will lay the foundations from which the republic will emerge. Thus, *The Prince* is "phase one" of a series of events that will lead to liberty and republican government in "phase two." On this account, *The Discourses* takes its point of departure from the (hoped-for) realization of the prince's plans. Put another way, *The Prince* has to do with "heroic" politics, *The Discourses* with mass politics of a republican sort, made possible by the heroism of the virtuoso leader. Thus Sheldon Wolin (1960, p. 231) contends that the prince will "render himself superfluous" and therefore "give way" to the rise of mass politics and the republic. Exactly how the founder renders himself superfluous or how he gives way to the republic, Wolin does not say; the implication, roughly, is that he creates institutions that will subsume and outlive him. Hence the major issue, for Wolin (1960, p. 231), is whether or not the state will be capable of "generating its own momentum" after the founder is dead.[4]

Two problems beset Wolin's attempt to solve the puzzle of the transition from *The Prince* to *The Discourses*, and so to shore up the third version of the strong republican thesis. The first problem is textual: in *The Prince*, Machiavelli gives no specific advice concerning the foundation of republican institutions. Indeed, he does not deal with republics at all. On this score, we should compare *The Prince* with a later work, Machiavelli's advice to Pope Leo X, "On Reforming the State of Florence" (Pansini, 1969, pp. 633-34). There he does offer lengthy and detailed directives on the organization of a republic, and advises, among other things, the reopening of the hall of the Council of the Thousand, the redistribution of offices to the general public, and the return of a gonfalonier. Nothing even remotely similar to this appears in *The Prince*, where Machiavelli seems content to develop Lorenzo's knowledge of historical examples and his appreciation of deceit and violence, rather than his familiarity with republican *ordini*.

The second problem with Wolin's thesis is more political in nature: Would a theorist as cognizant of the vicissitudes of *fortuna* as Machiavelli content himself with the notion that "heroic politics" will somehow "give way" to mass politics, that the death of the prince will lead to the rise of the republic? Moreover, would a Florentine who knew well the personalities of the Medici princes—Giuliano (a man of little ambition, with a lack of aptitude for dealing with Florentine affairs) and Lorenzo (an unapproachable autocrat with Spanish pretensions) (Gilbert, 1984, p. 135; Hale, 1977, p.

99)—truly imagine them fit subjects for heroic politics, much less men great-hearted enough to relinquish their power after creating the conditions for a new republic?

Surely these rhetorical questions answer themselves. The history of the government of Florence, with its unpredictable oscillations between various forms of princely and republican rule, taught Machiavelli at least one lesson: that to wait (as was the Florentine habit) and expect to benefit from circumstances brings disaster. What is necessary is to act and to act boldly to change circumstances. This, in fact, is the very lesson Machiavelli teaches his prince, so it seems odd, to say the least, to assume that Machiavelli himself is willing to wait for the prince's retirement or death and expect that circumstances will, in due course, eventuate in a republic.

These last observations give us the needed purchase, I believe, to understand Machiavelli's strong republican intentions and to solve the puzzle of *The Prince.* Given the Florentine's commitment to boldness and his conviction that successful political action requires the mastery of circumstances, does it not seem plausible that *The Prince* could be read as a political act in itself, a bold attempt to change existing conditions? Despite its main fault, the Rousseauian interpretation is compelling precisely because of this—it approaches *The Prince* as praxis and renders Machiavelli a political actor as well as a political theorist. Again, however, it must be remembered that Machiavelli sent his tract to the prince and not to Piero Soderini, the deposed gonfalonier, or to his republican friends in the castle at Volterra. So the puzzle remains. If we are to be committed to a reading of Machiavelli as a strong republican and of *The Prince* as praxis—as I propose we should—then in what sense is this little book a bold attempt to alter circumstances? How is the author of *The Prince* to be reconciled with the author of *The Discourses?*

These questions can be answered only if we remember Machiavelli's awareness of the advantages of crafty assault, and consider another, arguably more plausible, interpretative possibility: that *The Prince* is not simply about deception, but is itself an act of deception, and that this theorist of deceit is at the same time a practitioner of that very art. In other words, *The Prince* is a tract that in fact aims to restore a republic, though in appearance it dedicates itself to maintaining a princedom. Machiavelli indeed intends this book for the Medici. Thus, his deception resides not in exposing princely tricks to republicans, but in something far more crafty: he intends for a gullible and vainglorious prince to heed the duplicitous advice of *The Prince,* and thereby take actions that will jeopardize his power and bring about his demise. Thus, even as Machiavelli (*P,* p. 65) tells Lorenzo that "one who deceives will always find those who allow themselves to be deceived," he is deceiving Lorenzo. Even as he presses upon the prince the need to establish a relationship to others that is unknown to them, Machiavelli places Lorenzo in exactly this relation to himself. Even as he offers his Medici a "humble testimony of devotion," Machiavelli devises a plot, a series of moves that, if followed, will lead Lorenzo to disaster.

This reasoning presumes, of course, that Machiavelli was a decided enemy of the Medici, and that he intended his advice to be followed to its damning letter. We will turn to that shortly. First we need to consider how, in the course of *The Prince,* this master of political deception sets his trap, disguises his own true aims, and makes Lorenzo his *mannerino.*

Trapping the Prince

Machiavelli's conception of politics in *The Prince* is quite clearly drawn from his understanding of and experience in the art of war. Politics, like warfare, is a vicious struggle to gain control, to dominate and conquer opposing forces, to battle one's way to victory over the enemy. Machiavelli (*P*, p. 53) thus advises his prince to "have no other aim or thought . . . but war and its organization and discipline." That art alone is necessary for glory in politics. The organizer of the Florentine civilian militia also knows that though there is "no comparison" between the armed and the disarmed man, success in war depends upon more than brute assault by sheer force of arms (*P*, p. 54). Machiavelli (1965) recommends another sort of assault in *The Art of War:*

> Where the nature of the terrain is such that you cannot draw the enemy into an ambush easily, you may, however, dig ditches and pitfalls in the plains, cover them over lightly with brushwood and clods and leave areas of solid ground through which you may retire in the heat of battle; if the enemy pursues, he is undone. (1965, p. 118)

Thus, for the general and the prince, the art of war and the art of politics require a knowledge of crafty assault as well as of armed combat. The political actor must be as skilled at setting traps as he is at bold, ferocious attack, for when one is foiled by "terrain" and unable to ambush easily, it may be necessary to deceive.

The political terrain of Florence in 1512 was not advantageous for Machiavelli. No "easy ambush" of the Medici lords was possible, and therefore we might remember Machiavelli's advice to generals in such situations: employ strategy and deception and your enemy will be undone. *The Prince* is Machiavelli's stratagem, an act of assault in the form of deception. As has been recognized for centuries, the text itself provides areas of "solid ground," or firm advice a new prince in a new territory can rely upon to gain and maintain his power. What has been missed, however, is Machiavelli's deceit. Amidst this solid advice he prepares "ditches and pitfalls" in the form of subversive directives for his Medici lord, which he then covers over with promises of power, glory, and popular support. This deceptive advice to Lorenzo concerns three decisive matters for a prince: where to live, how to behave, and whom to arm. If we read Machiavelli's counsel to the prince with historical information at hand, its subversive character begins to appear. If we read it with a complete understanding of Lorenzo de Medici's circumstances in Florence, the conclusion seems obvious: Machiavelli is out to undo this enemy of the republic. Let us consider what he says.

Where to Live

Machiavelli begins *The Prince* by stating that his subject matter concerns monarchies, not republics (*P*, p. 5). This is surely disingenuous, for although his discussion of kinds of principalities and how they are acquired and kept (*P*, pp. 5-41) focuses primarily on princely power and not popular governance, one of his main categories of principality is the former republic. Machiavelli says that of all the new princes, the one who becomes ruler of a once-free city faces the most overwhelming difficulties, and he devotes a short chapter (*P*, pp. 18-19) to explaining princely options in such a situation. We, like Machiavelli, might expect that Lorenzo, a new prince in a former republic, would be particularly interested in this chapter, so we should note what Machiavelli prescribes when he addresses "the way to govern cities or dominions that, previous to being occupied, lived under their own laws" (*P*, p. 18). Machiavelli offers the prince in a formerly free state three choices: he may either despoil, live within, or restore the freedoms of the occupied city. He then discounts the third alternative by appealing to history: The Romans unsuccessfully tried to hold Greece and, at the same time, allow her freedom; hence, their only recourse was to lay waste and despoil the country as they had done in Capua, Carthage, and Numantia. The paradox is not lost on Machiavelli—in order to maintain power in a former republic, one must destroy it. What renders such extreme measures necessary is the character of the citizenry of a subjugated republic: "They do not and cannot cast aside the memory of their ancient liberty," Machiavelli warns, but then he concludes his chapter by offering the second option as another resort. If the prince can neither "lay waste" to the city nor restore its freedoms, then he must reside in it (*P*, p. 19).

Upon first reading, Machiavelli's advice in chapter 5 seems solid. Indeed, he does not even bother to defend it. In chapter 3, on "mixed monarchies," he argues that a prince's residence within his conquered territory renders possession "more secure and durable" (*P*, p. 8) and allows for the immediate remedy of disorder, and in chapter 6, he reiterates that the maintenance of power is facilitated by a prince "being obliged to reside personally in his own territory" (*P*, p. 21). Yet there is something curious, even contradictory, about his advice in chapter 5. Before he counsels residence, Machiavelli unequivocally states that "whoever becomes the ruler of a free city and does not destroy it, can be expected to be destroyed by it." A motive for rebellion against the prince, he argues, can always be found in "the name of liberty," which republican citizens cannot cast aside (*P*, p. 18).

If this is the case, then what should we make of his advice? It seems that a prince who lives within a conquered republic would stand to lose rather than benefit, particularly if the people have not forgotten "the name of liberty." A prince within a city is easier to find and destroy than one who lives in a country villa, as had been the habit of the Medici family; they maintained a palace within Florence but spent much of their time in their villas—Careggi, Cafaggiolo, Castello, Fiesole, Poggio a Caiano—outside the city (Burkhardt, 1958, p. 399; Wackernagel, 1981). Machiavelli's advice seems designed to change the residential practice of the family by strategically placing the prince inside the city's walls. Yet at the same time it seems to run counter to his warning about the vengeful nature of former republicans—why should a prince live in their midst?

Perhaps our perspective is not yet complete. Machiavelli may be determined to assure Lorenzo's power by offering further advice on how to neutralize the "desire for vengeance" and the love of liberty that inflame republican hearts, so that even though the prince resides within the city, he will be secure. Machiavelli does give advice on this score, but what he says is curious indeed.

How to Behave

If any one piece of advice occurs repeatedly in *The Prince,* it is Machiavelli's dictum that the ruler should always strive to gain the favor of the people. In chapter 9, "Of the civic principality," Machiavelli tells Lorenzo to reject the "trite proverb" that, "He who builds on the people builds on mud," for the prince who animates the masses will find "he has laid his foundations well" (*P*, p. 38). Machiavelli reiterates this point in various ways throughout the treatise (*P*, pp. 60, 61, 63, 67, 71, 75, 76, 80). Because the friendship of the people is the prince's "main resource" in times of adversity, he must avoid incurring their hatred in order to insure against conspiracy or ruin; this is "one of the most important matters a prince has to confront" (*P*, p. 67). Machiavelli acknowledges that princes cannot always avoid being hated by someone, but it is best if those who hate him not be the people (*P*, p. 67). In a new age, then, all princes (except the Turk and the Sultan) ought to aim at satisfying the *popolani.* Implicit is a corollary Machiavelli makes explicit in his chapter on civic principalities: be wary of the nobility. The nobles are portrayed as untrustworthy, dishonest, greedy, and dangerous. Machiavelli writes:

> [F]rom hostile nobles [the prince] has to fear not only desertion but their active opposition, and as they are always more far-seeing and more cunning, they are always in time to save themselves and take sides with one who they expect to conquer. (*P*, pp. 36-37)

Hence the wise prince will, when possible, esteem his nobles, but more often he will be suspicious of them. The people provide a far firmer foundation for power.

Before we accept the astuteness of Machiavelli's advice to Lorenzo, we should recall this Medici's situation. As we shall see, the Florentine people were not well inclined toward the new Medici. The mood of the city had changed markedly since the days of *il Magnifico*. The Florentines had become accustomed to a republic; what opposition there was to it came primarily from the aristocracy. It seems, then, that an astute advisor would have told Lorenzo to turn for support to the very class Machiavelli tells him to suspect—the *ottimati.* All the more curious is Machiavelli's own acknowledgement of this elsewhere (though not to Lorenzo in *The Prince).* In his document to Pope Leo X, "On Reforming the State of Florence," Machiavelli tells the Pope what would have to be done if a prince wished to turn the city into a monarchy:

> [I]n Florence where there is a great sense of equality, one would first have to introduce inequality and create nobles with castles and villas, who would join the prince in suppressing the city and the whole province with their armies and factions. A prince alone, without the nobles, cannot bear the weight of a monarchy. (Pansini, 1969, p. 20)

Despite the fact that *The Prince* is (ostensibly) dedicated to a prince who wishes to maintain his power, nowhere in the treatise does Machiavelli offer Lorenzo the advice he gives Pope Leo. In fact, he says exactly the opposite and issues warnings about trusting nobles and alienating the people. Of course, it may be that between 1512 and 1521 Machiavelli simply changed his mind on the subject of whose favor the prince should seek, but before we draw this conclusion we might look at another aspect of his advice on how to behave.

Machiavelli takes up the subject "Of liberality and niggardliness" in chapter 16. In advising the prince on how to behave, he again reminds him of the importance of not being hated by the people, observing that

> one who wishes to obtain the reputation of liberality among men, must not omit every kind of sumptuous display, and to such an extent that a prince of this character will consume by such means all his resources and will be at last compelled, if he wishes to maintain his name for liberality, to impose heavy taxes on his people, become extortionate, and do everything possible to obtain money. This will make his subjects begin to hate him. (*P*, p. 58)

To underscore his warning against liberality *(liberalita),* Machiavelli concludes the chapter by saying, "of all things that a prince must guard against, the most important are being despicable or hated, and liberality will lead you to one or other of these conditions" (*P*, p. 60). Thus, the prince (who has already noted the danger of alienating the people) will agree to practice "niggardliness" *(parsimonia)*—it is one of the vices that secures his power. However, we have evidence to suggest that Machiavelli's warning against liberality in *The Prince* is more a matter of republican sympathies than helpful advice, for in *The History of Florence* he reveals how the Medici benefitted from liberality.

Book 8 of Machiavelli's *History* relates, among other things, the famous tale of the Pazzi conspiracy against *il Magnifico,* Lorenzo de Medici, in 1478. The details of the attempted but unsuccessful plot against Lorenzo's life need not concern us, but what Machiavelli says about the outcome of the conspiracy is instructive. One of the elements of the plan called for Francesco Pazzi to ride to the gates of the city in the aftermath of Lorenzo's assassination, calling the people to liberty and to arms. However, events took a different turn. Francesco was wounded in the attempt to kill the prince, and it fell to his uncle, Iacopo, to sound the alarm. Machiavelli explains and analyzes the failure of this final effort by the Pazzi in the following way:

> Iacopo rode out with perhaps a hundred men who had been prepared for this job, to make this last trial of their fortune, and he went to the Palace square calling the people and liberty to his aid. *But the first had been deafened by fortune and the liberality of the Medici* and the second was unknown in Florence, so there was no response. (1970, p. 273, emphasis added)

It is impossible to miss Machiavelli's conclusion—the liberality of the Medici had garnered the people's support. Indeed, the family's practiced munificence and heavy public spending had deafened the Florentines to the *cri de coeur* of the republic—that is, to liberty.

Armed with Machiavelli's analysis of this event, we can now read his advice in chapter 16 of *The Prince* in a different light. His injunction against liberality is intended to deprive Lorenzo of a tactic that had worked exceedingly well for the Medici in the past. As Machiavelli sees it, a liberal prince can spend, then depend on the people's goodwill, a miserly one cannot. It seems, then, that even as he assures Lorenzo that miserliness will win him public support and keep him from being hated, Machiavelli takes steps to guarantee that the name of liberty is not forgotten in Florence—and it is in liberty that citizens of a former republic "can always find a motive for rebellion" (*P*, p. 19). Yet, as Machiavelli knows, rebellion may come to naught unless there are good arms to strengthen it. We must now consider his advice on arms.

Whom to Arm

In chapter 20 of *The Prince*, Machiavelli takes up the matter of "whether fortresses and other things which princes often contrive are useful or injurious." Allan Gilbert (1938, p. 162) has noted this chapter's "un-Machiavellian" advice to the ruler about how to gain the support of the people, but let us take another look. The chapter begins with a review of what princes who want "to hold their possessions securely" have done, and Machiavelli notes that no "definitive judgment" is possible on these matters (*P*, p. 77). Conditions and circumstances vary and require different responses. But, then he issues a most definitive statement:

> A new prince has never been known to disarm his subjects, on the contrary, when he has found them disarmed he has always armed them, for by arming them these arms become your own, those that you suspected become faithful and those that were faithful remain so, and from being merely subjects become your partisans. (*P*, p. 77)[5]

As if to leave no further doubt on the matter, Machiavelli goes on to argue that the new prince who disarms his subjects offends them and generates hatred toward himself. As we have seen, he has taken care to prepare the solid ground for this advice by telling the prince that the hatred of the people is precisely what he must avoid; one of his most potent remedies against conspiracy is not being hated by the masses. Therefore, Machiavelli concludes, "a new prince in a new dominion always has his subjects armed. History is full of such examples" (*P*, p. 78). His advice on this score follows from chapter 14, where the prince has been warned of the evils of being disarmed, and from chapter 12, where the prince has been told of the disastrous consequences of hiring mercenary and auxiliary troops.

On the surface, perhaps, Machiavelli's advice on arms seems sound, even "un-Machiavellian," but that impression begins to blur if we consider a seemingly obvious point. Machiavelli's suggestion that the granting of arms inspires loyalty and makes partisans out of subjects, if taken to be sincere, fails to account for the possibility, indeed the probability, that arms may also facilitate plots, incite insurrection, and inspire rebels. The new prince who arms his subjects may just as easily make himself a mark for overthrow by creating the very instrument of his own destruction, namely, a civilian militia. Moreover, earlier in the treatise, when Machiavelli discusses such virtuous new princes as Francesco Sforza in Milan or Cesare Borgia in the Romagna, he makes no mention of their having armed their subjects, doubtless because they did not. His bold claim that "history is full of such examples" is followed by no examples at all, an odd omission for a thinker who is otherwise so willing to present specific historical examples for the prince to emulate.

Most curious of all, however, is Machiavelli's omission of a historical example that would have meant much to Lorenzo. In a letter written to Piero Soderini at the same time he was composing *The Prince*, Machiavelli (1961) makes mention of some causal histories that have led various princes to greatness, and notes that

> Lorenzo de Medici disarmed the people to hold Florence; Messer Giovanni Bentivogli in order to hold Bologna armed them; the Vitelli in Castello and the present Duke of Urbino in his territory destroyed the fortresses in order to retain their states; Count Francesco and many others built them in their territories to make themselves sure of them. (1961, p. 98)

Machiavelli cites the Bentivogli, the Vitelli, and Francesco Sforza in his discussion of the things princes contrive in chapter 20, but he never acknowledges the example of Lorenzo de Medici, a new prince who disarmed his subjects in order to hold them. Surely this is odd, for of all the historical examples for the younger Lorenzo to emulate, his grandfather would have been the best. Not only was *il Magnifico* the most artful *principe* of the Medicean line, he was also the most brilliant secular figure of the age, both loved and feared by the Florentines. But quite clearly, the example of Lorenzo the Magnificent directly contradicts Machiavelli's advice on arming subjects, and Machiavelli appears content to omit this piece of information in his "definitive" advice to the new prince.

The issue here, however, is not simply whether new princes have in the past routinely armed their subjects and, in essence, created civilian militias. At issue is another, more immediately historical matter: is Machiavelli's advice wise counsel for a Medici in Florence? From what we know of the history of the city, the answer to this question can only be no.

In Florence, the idea of liberty was deeply rooted in political tradition. The city's sense of freedom persisted through periods of oligarchical rule, rigged elections, and partisan foreign policies. A proclamation of July 1329, passed by the *pratica* (a citizen assembly), declared that the city would never submit to the autocratic rule of one man, "since liberty is a celestial good which surpasses all the wealth of this world" (Rubinstein, 1968, p. 450). That "celestial good" was what the Florentines believed contributed to their greatness. Coupled with this tradition of republican liberty, the Florentines had a history of strong opposition to the Medici, which grew particularly virulent in the mid-fifteenth century. In an oath sworn in May of 1466, 400 citizens over the age of 14 declared a political program of opposition to Medici rule that demanded, in part, that citizens "be free to debate and judge public and popular government" (Rubinstein, 1968, p. 458). Nor were the Florentines without individual voices raised in defiance of the Medici usurpation of liberty. In his

Laudatio Florentinae Urbis, Leonardo Bruni declared, "nothing can be achieved by the covetousness of single citizens against the will of so many men," and suggested safeguards against autocratic power (Rubinstein, 1968, p. 446). Alamanno Rinuccini published *De libertate,* a powerful attack on Medici tyranny (Rubinstein, 1968, pp. 461-62), and Girolamo Machiavelli, Niccolo's great-granduncle, was tortured and executed for his part in leading the opposition to Medicean measures instituted under the rule of Cosimo (Machiavelli, 1970, p. 218; Ridolfi, 1963, p. 2).

There is no reason to suspect that this opposition to the Medici or the spirit of Florentine republicanism itself had softened or disappeared by 1512, or that Machiavelli was unaware of it. Without question, the republic was anything but stable and secure; internal struggles between the middle class and the aristocracy were unending, making institutional reform immensely difficult and external affairs precarious. By 1510, Piero Soderini, the gonfalonier, was out of favor; the aristocracy was pressing for his removal from office and for a return to *governo stretto,* though not for an end to the republic. There is, in fact, little evidence to suggest that there was any popular sentiment favoring the return of the Medici, nor is there any indication that the Medici's assumption of power was generated in any major way by forces within the city.[6] No doubt the family had allies among a small number of wealthy families and some younger aristocrats who stood to gain commercially and politically from the decline of the republic (Brucker, 1969), but in the end, the Medici resumed their power with the aid of Spanish bayonets, not the Florentine citizenry (Gilbert, 1984; Hale, 1977; Schevill, 1936). The fact that, once installed in power, the princes kept foreign troops in the city and guards at the palace indicates that they felt some uncertainty about their popularity. Perhaps this is why the younger Lorenzo, unlike his grandfather, *il Magnifico,* rarely ventured into public places to mingle and meet with the citizens. When he did, he was accompanied by armed guards (Gilbert, 1984, p. 108).

With respect to the matter of arms and Florence, let us further consider Machiavelli's advice to Lorenzo on fortresses, for it is as curious as his advice on arming the citizenry. In fact, his injunction against a fortress, when read in light of Florentine republicanism, also seems far more in keeping with the interests of the republic than with those of the Medici.

In chapter 20, Machiavelli tells Lorenzo that "a prince who fears his own people more than foreigners ought to build fortresses, but he who has greater fear of foreigners than of his people ought to do without them" (*P,* pp. 80-81). This is clever strategy, considering that Machiavelli has previously condemned the prince who fears his own people, and has stressed the danger of placing too much trust in foreign powers. Hence, the prince who can read at all cannot help but decide, as Machiavelli would have it, not to build a fortress. More metaphorically, the best fortress is to be found in the "love of the people" (*P,* p. 81).

Militarily, Machiavelli's counsel on fortresses runs counter to the prevailing views in Tuscany in the early Renaissance. Fortresses were considered useful in defending a dominion from outside enemies. In opposition to this, Machiavelli argues that a *fortezza* is useless and cites the example of the Castello Sforzesco, which gave "more trouble to the house of Francesco Sforza than any other disorder in the state" (*P,* p. 81).[7] At best, Machiavelli's military analysis of this matter is exceedingly thin. He seems more interested in the internal, political implications of this strategy, rather than the external military ones. Again, we must examine what such an internal strategy would mean in the context of Florence under a Medici lord.

If the tradition of liberty distinguished Florentine republicanism in a political sense, then the walls of the city marked her republicanism in a strategic one (Hale, 1968, p. 502). The Florentines were notoriously wary of the subject of fortresses within the city. A *fortezza* symbolized the antithesis of republicanism, signaling the demise of popular governance and the emergence of an inner power elite by providing an autocrat with an impregnable stronghold. Observing the circumstances of neighboring city states, the Florentines saw that princes often constructed fortresses in the name of military security, but in fact used them for purposes of domestic oppression. Not surprisingly, then, the Florentines, with their strong republican traditions, viewed the building of a fortress as both a symbolic and a literal danger.

Machiavelli shares this suspicion. In a letter to Guicciardini, written in 1526, he equates the building of a fortress with the enslavement of the Florentines and warns that "the most harmful thing a republic can undertake is to enact something strong or that easily can be made strong within its body" (Machiavelli, 1961, p. 235). He goes on to observe that, if a fortress existed in Florence, "any powerful man" who conquered the city would, upon entering, find it a convenient stronghold, and the Florentines "would become slaves without any protection" (Machiavelli, 1961, p. 235).[8] These observations raise yet another puzzle. If Machiavelli is so convinced of the danger a fortress poses to a free Florence and of the advantages it holds for a prince, then why, when devising a strategy for the Medici, does he not recommend the building of a *fortezza?* Indeed, what would a Medici prince who is backed by Spanish troops in a city under siege stand to gain by not fortifying himself? Yet in the face of such facts, and knowing the problems a fortress would present for republican activity, Machiavelli does not recommend that Lorenzo build one; nor does he recommend any strategies of "containment" other than the "love of the people." How can we account for this puzzling advice on arms and fortresses?

The mystery or oddity of Machiavelli's treatment of arms and fortresses can be explained in only one way. He offers Lorenzo advice on security, with the intention of delivering him into republican hands. Machiavelli has not lost sight of the reality of Florentine politics; he knows full well what the consequences will be if Lorenzo resides in the city, foregoes liberality, arms the people, distrusts the nobles, refuses a fortress, and mingles "from time to time" with the Florentines. In a city where "the desire for vengeance" runs deep and "the memory of ancient liberty" shines bright, an "unarmed prophet" is never fully secure; an unwanted prince who arms his subjects and does not protect himself is even less likely to survive. As his "advice book" proceeds, Machiavelli's warning in chapter 5, "whoever becomes the ruler of a free city and does not destroy it, can be expected to be destroyed by it," (*P*, p. 18) takes on the character of prophecy. Lorenzo will not destroy Florence; that much is clear. His only alternative (if he takes Machiavelli's counsel) is to reside in the city. Once therein, Machiavelli will have him adopt policies that are, in fact, republican snares designed to entrap him. He will be destroyed. These are the "ditches and pitfalls" that lie beneath the seemingly solid ground of Machiavelli's advice to his Medici lords.

Machiavelli and "The New Sons of Brutus"

If I read it aright, *The Prince* is itself an act of political deception. In addition to the deceitful "advice" in the text itself, signs of crafty assault are also to be found outside Machiavelli's text, and all point in only one direction: not only was Machiavelli no friend of the Medici; he was a decided enemy. He believed that the Florentine republic had to rid itself of their control if it was to revive and flourish. Other documentary sources and Machiavelli's own political biography help shore up my case.

Among Machiavelli scholars, it is generally agreed that in 1513 he was concerned with getting into the Medici's good graces, finding employment, and returning to active political life. According to Quentin Skinner (1981, p. 21), "As soon as he came out of prison, Machiavelli began scheming to recommend himself to the city's new authorities." To insure the success of his scheming, Machiavelli wrote numerous letters to his friend, the erstwhile republican Francesco Vettori, now the Medici's ambassador to Pope Julius II and Machiavelli's only access to the corridors of power. At every turn, the political exile appears eager to ingratiate himself to the Medici and, through Vettori, to assure the family of his trustworthiness, his honesty, and his support. In the famous letter of December 10, 1513, Machiavelli (1961, p. 142) tells Vettori of his "little work" *On Princedoms* and of his intention to offer it to the new prince, Giuliano, in the hope that the prince will welcome it and make use of the talents of the author. The dedicatory pages of *The Prince* seem to provide further evidence that Machiavelli was concerned with promoting the greatness of the Medici and with assuring their success through the benefit of his knowledge of the "art of the state."

That Machiavelli was a schemer is beyond doubt. Nor does there seem to be any question that he was eager to have *The Prince* gain the eye and approval of Giuliano and, later, Lorenzo. However, those who are tempted to read *The Prince* as so much Machiavellian opportunism would do well to consider what Machiavelli has to say in *The Discourses* about "the sagacity and severity of Junius Brutus," the "father of Roman liberty" (*D*, p. 402). We will return to the severity of Brutus shortly. Let us first consider his sagacity.

In book 3, chapter 2 of *The Discourses*, Machiavelli notes that more than anyone else, Junius Brutus deserves to be esteemed for his "simulation of folly" (*D*, p. 403). Having neither the arms nor the forces to mount an open war against the kings of Rome, Brutus instead ingratiated himself with those in power. On this matter, Machiavelli takes issue with Livy's interpretation of Brutus's motives and suggests a reading of his own:

> Titus Livius gives but one reason that induced Brutus to this simulation, namely, that he might live in greater security and preserve his patrimony, yet if we well consider his conduct we are led to believe that he had another reason, which was that by thus avoiding observation he would have a better chance of destroying the kings and of liberating his country, whenever an opportunity should offer. (*D*, p. 403)

Clearly, Machiavelli not only holds Brutus in great esteem for his simulation of folly, but also for his commitment to liberating his country, and he offers Brutus as a model for others who are "dissatisfied with their ruler." In Machiavellian terms, if one cannot play the lion and threaten the prince directly by force of arms, then one must be a fox and undermine the enemy from within. "It is advisable . . . at times to feign folly, as Brutus did," Machiavelli writes, "and this is sufficiently done by praising, speaking, seeing and doing things contrary to your way of thinking and merely to please the prince" (*D*, p. 404).

Interpreters of Machiavelli should be wary of repeating Livy's mistake about Brutus, and so avoid being tricked by appearances. If Machiavelli had designs on the Medici, as I think he did, then it seems reasonable to expect that he would seek to prove himself a trustworthy advisor, and so get the Medici to follow his advice. If we keep Machiavelli's praise of Brutus's sagacity in mind, then his own flattery of Lorenzo and his appeals to Vettori seem even less likely to be matters of simple opportunism, and rather more deeply political. Like Brutus, Machiavelli trades on the appearance of friendship and admiration for his ruler; he "feigns folly" for a political purpose—to ingratiate himself to Lorenzo, thereby earning his trust so that the prince might be destroyed—much as Junius Brutus destroyed the kings and restored the liberty of Rome.

There are other reasons to question whether Machiavelli was so monomaniacally consumed with getting his old job back that he simply shed his republican sympathies and dedicated himself to serving Florence's Medici lords. First, there is the political history of the Machiavelli family itself. Though family legacy need not determine one's loyalties, it generally serves as a source of personal identity as well as of reflection, particularly in an age where a family's identification with the political order is of great and lasting importance. Machiavelli himself points out in a chapter in *The Discourses*—straightforwardly entitled "Reasons why the same family in a city always preserves the same character"—that what a youth hears praised or censured within his family "becomes afterwards the rule of his life for all time" (*D*, p. 535). As Machiavelli's biographer tells us, the Machiavelli were an old Florentine family, noted for their active participation in Florentine public life and, most significantly, for their devotion to the republic. They gave the city some 12 gonfalonieri and 54 priors (Ridolfi, 1963, p. 2). As I have noted, Girolamo Machiavelli died in prison because he dared to speak out against the oligarchy of Cosimo de Medici, and another Machiavelli, Francesco, was remembered for a public speech in 1424 that condemned tyranny and praised liberty: "The enjoyment of freedom makes cities and citizens great: this is well known. But places under tyranny become deserted by their citizens and engage in their extermination" (Pitkin, 1984, p. 204). Whether Machiavelli knew of Francesco's speech is uncertain, but its sentiments were in all likelihood passed on, for they are clearly evocative of some of the central tenets of *The Discourses*.

In short, there seems little question that the Machiavelli family viewed Piero de Medici's ouster in 1494 (when Niccolò was 25) and the revival of republicanism more favorably than did the aristocratic Tournabuoni, Guicciardini, Strozzi, or Neroni families, who feared for their interests under a radically republican regime. Also, the sentiments of Florence itself were generally more "Machiavellian" than aristocratic. Piero's three attempts to return forcibly were met with armed resistance from the citizenry. Effectively defeated in 1498—the year Niccolò was appointed Second Chancellor—the Medici finally ceased plotting against the republic, retired to their country villas, and left *fortuna* to do what they could not.

In compiling evidence of Machiavelli's animus toward the Medici, we must, secondly, consider some crucial events in his own life, as well as the wary view the Medici took of him. Although the family was a suspicious lot, they were not wholly undiscriminating in their purge of republicans. Thus, when the chancery was reorganized in 1512, the First Chancellor, Marcello Virgilio, was allowed to remain in office (Hale, 1961, p. 134). In fact, Machiavelli was the only chancery official dismissed by the Medici (Gilbert, 1984, p. 173).[9] Without doubt, the Medici regarded Machiavelli with considerable suspicion. As we know, nearly a decade later they granted him a modest stipend to write his Florentine history, but even though his literary skills were gaining fame, his political trustworthiness was still very much in doubt. Indeed, the family never allowed him back into any position of political power.

We must also remember the Boscoli-Capponi affair. A few months after his removal from office, Machiavelli was implicated in a conspiracy against Giuliano when his name was found on a list in the possession of one of the two young conspirators. The Medici imprisoned and tortured him on the rack but found out nothing (Mattingly, 1958, p. 485). Machiavelli may have been ignorant of the plot, but it is a significant political fact that the anti-Mediceans surely perceived him to be a potential ally, one who might be counted on for support had the assassination succeeded.

Nine years later, in 1522, Machiavelli again fell under suspicion following the abortive plot to assassinate Cardinal Giulio de Medici. The plot was organized by two of his closest friends, Zanobi Buondelmonti (to whom he had dedicated *The Discourses* a few years earlier) and Luigi Alamanni, a poet and fellow member of the Oricellari circle. In 1524, one of the accomplices revealed that Buondelmonti had mentioned Machiavelli's name as one of several citizens who should be invited to join the plot (Ridolfi, 1963, p. 203), but by then the dust had settled. Buondelmonti and Alamanni had fled the country, two others had been executed, and Machiavelli was in refuge in San Casciano, writing his Florentine history.

For all we know, Machiavelli may have or may not have been involved in either or both of these conspiracies. Assuming, conservatively, that he was not, we can read this noninvolvement either as a sign that he was loath to conspire against and more willing to work with the Medici in order to stabilize Florence, or as a sign of his wariness about the success of conspiracies in general, even if he wanted to see the Medici removed from power. What Machiavelli writes appears to substantiate the latter view, at least in part. His famous chapter on conspiracies in *The Discourses* (which Buondelmonti must have read), though filled with advice on how to conspire, is replete with warnings about the slim chances conspirators have for success (*D*, pp. 410–36). And in his history, Machiavelli writes, in reference to attempted assassinations of the Medici, that "because conspiracies rarely succeed, they most often bring about the ruin of those who plan them and they bring greatness to those against whom they are directed" (*D*, p. 263). We might expect, then, that Machiavelli's reluctance (if reluctance it was) to participate in conspiracies owed more to caution than to allegiance. Indeed, his republican friends must have taken his noninvolvement in their conspiracies (if again, noninvolvement it was) in this latter light, for in 1527, when the Medici were once again forced from Florence, Buondelmonti and Alamanni, now returned, supported their friend's appeal for employment in the restored republic, at a time when the "enemies of the republic," Guicciardini and Vettori among them, were being called into court (Gilbert, 1984; Ridolfi, 1963).

Apart from *The Prince*, Machiavelli's writings provide us with some final clues about his attitude toward the Medici. What he writes in reference to the family and their supporters is decidedly negative, although his true opinion is not always immediately apparent—nor could it be. These were treacherous times and one risked imprisonment, or worse, if one spoke too critically of the Medici lords or their policies. Political tracts and correspondence had to be crafted slyly; an enemy of the regime had to know how to appear to be loyal, even though he actually was not.[10]

Machiavelli practices this political strategy in the *History of Florence*. The work was not simply an intellectual endeavor; there were political implications as well, since it had been commissioned by Cardinal Guilio, a member of the Medici family. Machiavelli was not unaware of the delicacy of his task. He wrote to Guicciardini asking for advice on what to say about the family and how to say it. Then he concluded, "I . . . shall try to act in such a way that, since I tell the truth, nobody will be able to complain" (Machiavelli, 1961, p. 206). What we find in the history, particularly in the sections devoted to the Medici, is truth telling of a particularly "Machiavellian" kind. There are numerous flattering references to the Medici, to be sure. Machiavelli calls Cosimo, "the most famous and respected citizen . . . who ever lived," describes Piero de Medici as a man of "*virtù* and goodness" and a great citizen, and extolls Lorenzo the Magnificent's beneficience, his contribution to the city, his dignity, and his property, "worthy of a King" (Machiavelli, 1970, pp. 220, 246, 315-17). In short, Machiavelli's truth telling seems designed to cast the Medici in the best possible light, and to play to the cardinal's family pride.

A closer reading, however, reveals a different sort of truth telling. If we put Machiavelli's assessment of the Medici princes in the context of some of his more general comments on the corruption of Florence, the family does not fare so well. At the start of book 7, where he shifts from a consideration of foreign affairs to "troubles at home," Machiavelli notes that the unity of a city is threatened most by the presence of factions and partisans. Citizens, he says, can make a reputation either publicly or privately. The latter course is reprehensible, for "this sort of behavior gives rise to factions and partisans, and a reputation won in this way is as offensive as the other kind is valuable" (Machiavelli, 1970, pp. 214-15). The tactics employed by private citizens include doing good to various other citizens, helping someone with money, getting him undeserved honors, and beguiling the lower classes with public benefactions and amusements. In short, the private citizen creates a coterie of "hangers-on," as Machiavelli puts it, in order to advance his own personal goals. Machiavelli's tone indicates his contempt for such activity and for citizens such as these, who contribute to the corruption of cities like Florence.

We need only match Machiavelli's description of the offensive behavior of private citizens with his ostensible praise of the Medici to see that in extolling the princes he is actually condemning them. By his account, the Medici are private citizens; they corrupt the republic by playing to factions. Liberality was Cosimo's forte. We have already seen that Machiavelli thinks liberality deafens a people to the cry of liberty and lulls them into forgetting their freedom. Lorenzo dispensed pensions and aid to the nobility and put on tournaments and pageants for the populace, tactics used by the private citizen in order to foster partisan support. It is Machiavelli's lengthy account of Piero's short-lived regime, however, that best captures his aversion for Medici maneuvers, for he gives there a detailed picture of the ravages of factional and party circumstances that plagued Florence during the Medici years (1970, pp. 228-41). In various ways, then, Machiavelli shows us how circumstances at once necessary for this family's perseverance were profoundly destructive of Florence herself.

Guilio's (now Pope Clement) enthusiastic acceptance of this historical masterwork suggests that Machiavelli achieved his goal—writing a truthful account in such a way that nobody complained. He had also, however, registered a distinctively "Machiavellian" conviction, one that took the form of subtle historical analysis: in order to survive, the Florentine republic must rid itself of the Medici.[11]

We might turn to one final piece of

written evidence. Machiavelli's conviction that the Medici were a threat to Florentine liberty surfaces in *The Discourses* as well, and returns us to his discussion of Junius Brutus. If Brutus's sagacity enabled him to restore the republic of Rome, his severity helped him to maintain it. The father of the Roman republic condemned his own sons to death because they were the enemies of liberty, even as he was liberty's defender. The lesson Machiavelli (*D*, p. 405) draws from Brutus's example is a characteristic one; whoever makes himself a tyrant and fails to kill Brutus, or restores liberty to a former republic and fails to kill the sons of Brutus, will not maintain himself for long. This seemingly nonpartisan advice is followed by an example, that of Piero Soderini, the deposed gonfalonier of Florence. Soderini realized that the "new sons of Brutus" had to be destroyed, but, as Machiavelli (*D*, p. 405) writes, he did not have the courage to do it. However, Machiavelli's story of Soderini is more than just a counterexample of the severity of Brutus; what makes it interesting is the moral gloss he adds after telling the tale of the fallen gonfalonier:

> [O]ne should never allow an evil to run on out of respect for the law, especially when the law itself might easily be destroyed by the evil and [Soderini] should have born in mind that as his acts and motives would have to be judged by the result . . . everybody would have attested that what he had done was for the good of his country and not for the advancement of any ambitious purposes of his own. . . . But Soderini was the dupe of his opinions, not knowing that malignity is neither effaced by time, nor placated by gifts. So that by failing to imitate Brutus he lost at the same time his country, his state and his reputation. (*D*, p. 406)

Machiavelli's harsh judgment of Soderini can be read cynically, of course; the gonfalonier's decline was Machiavelli's as well, and he had reason to resent it. However, the nature of Machiavelli's criticism—his reference to the "evil" that threatened the country and the "malignity" that would not disappear over time—suggests there is more at stake for him here than the mere salvaging of his reputation. When evil threatens liberty, hopes hang on the sagacity and severity of those who revere the republic. Brutus met this test; Soderini failed it, and so Florence lost her freedom. In Machiavelli's eyes, Soderini's great weakness was that he did not love the republic enough to act boldly and eliminate her enemies before they could conquer Florence and obliterate liberty.[12] Machiavelli's disdain for Soderini is outweighed only by his hatred for those whom Soderini could not destroy—those who threatened liberty—the "new sons of Brutus," the Medici and their allies.

The Prince in Perspective

I began by suggesting that Machiavelli's genius resides in his appreciation of crafty assault in all its guises—the lover's trickery, the general's stratagem, the prince's artifice—but in truth, Machiavelli directs his most penetrating attention to the actor/advisor behind the assault, the one who, observing the scene from a distance, controls the lover, the general, the prince. That is, what Machiavelli admires is a kind of Renaissance artistry—a strategic perspective—that allows for a unique conception of space or terrain, and consequently makes possible the manipulation of persons and events. Ligurio employs just such a perspective in *Mandragola;* Fabrizio exhibits it in his topographical advice to generals in *The Art of War*. But *Mandragola* and *The Art of War* are simply later versions—one dramatic, the other military—of the strategic perspective Machiavelli himself practices as advisor to the prince.

We might measure Machiavelli's success as a political advisor who sees things strategically by the labels he has earned—"political realist" and "master of *realpolitik*"—and by the statesmen and politicians, from Metternich to Kissinger, who have adapted his strategic perspective to their own times and circumstances. Yet to stop here, as do so many of the political scientists and political actors who cite him, is to overlook a matter of deepest importance to Machiavelli, namely, the political values—republicanism and liberty—that inform his perspective as advisor to Lorenzo de Medici. When, in the introduction to *The Discourses*, Machiavelli (*D*, p. 101) tells his friends to "[look] rather to the intention of him who gives than to the thing offered," he implicitly underscores the distinction between the apparent meaning of any given work, political counsel, or strategic perspective and its deeper purpose. To put this more broadly, political advising involves more than the capacity to analyze events or to see things as they "really are," for the "reality" that informs the analysis is neither a neutral observation nor a scientific truth, but a perspective colored by the values, purposes, and political commitments of the advisor who offers them. When these are at odds with the interests of those in power, the advisor may choose to retire, to capitulate, to oppose openly, or (as Ligurio puts it) "to pursue deceit to its envisioned dearest goal" (Machiavelli, 1957, p. 38) by painting a particular vision of reality for the ruler whom he or she counsels, with a precise purpose in mind. The latter is Machiavelli's strategy as advisor in *The Prince;* his purpose is the restoration of republican liberty.

I want to argue, then, that there is more involved in Machiavelli's advice in his little treatise than a presentation of *realpolitik*, but at the same time suggest that his deeper purpose—to deceive—is not at odds with reading *The Prince* either in terms of a new science or as a work of Renaissance artistry. By way of clarification, let us consider how Machiavelli himself depicts his perspective in *The Prince* and thereby gain one final interpretative clue to his aims and intentions.

In his dedication to Lorenzo, Machiavelli consciously invokes Renaissance artistry in its most literal sense, and draws an analogy between himself, the advisor to princes, and the landscape painter:

> [F]or in the same way that landscape painters station themselves in the valleys in order to draw mountains or high ground and ascend an eminence in order to get a good view of the plains, so it is necessary to be a prince to know thoroughly the nature of a people and one of the populace to know the nature of princes. (*P*, p. 4)

By inviting us to recall the great innovation of Florentine painting—its attention to accurate representation of pictorial space—Machiavelli also discloses a necessary quality of the political advisor. The intellectual disposition, or, to return to Machiavelli's visual metaphor, the "vantage point" of the advisor, must be fully dimensional and complete. It encompasses the actors and influences that populate and permeate the vast political landscape, and thus avoids the restricted perspectives of the prince or the populace, whose visions are governed solely by their respective relationships to one another. Unlike the actors he observes, the advisor stands "outside" the political canvas and integrates particulars into a sweeping contextual vision of reality. He sees actors not as isolated figures and events not as disconnected instances, but as parts of a richly constituted tapestry, a variegated field of competing interests and ambitions. The advisor's special disposition and imagination are, then, the very opposite of the short-sightedness Machiavelli deplores as the mark of politically ineffectual men, those who cannot control events or see beyond their immediate circumstances.[13]

But, as I have argued, there is more involved in Machiavelli's advice than a detached depiction of political reality, just as there was more involved in the Renaissance art of perspective than the achievement of pictorial veracity. In a literal sense, the discovery of perspective also introduced the art of deception to Renaissance painting. Though the depiction of reality was a central concern for the painter, it did not involve a simple mirroring of the physical universe. Rather, the painter selected particular elements of the visible world, then arranged and conveyed them so as to give the illusion of reality (Brucker, 1969). This act of artistic deception involved more than a dazzling display of technical virtuosity; also vital to it was the relationship the painter forged between the illusion he presented and the person who observed it. Thinking of his canvas as a window through which he viewed the world, the artist sought to convey the illusion as reality, and to stimulate the observer's emotions and sense of possibility (Ackerman, 1969; Brucker, 1958). From "inside" the painting, the artist tantalized his observers with a seductive vista and pulled them toward a point or prospect that seemed attainable. At its most powerful, perspectival art induced the viewers into actually feeling a part of the painting, as though they could step into it and secure the prospect that beckoned them. The artist performed a feat of aesthetic manipulation even as he accomplished an act of pictorial veracity. He used his science to produce a material work of deception, an art.

In much the same way as the art of deception is a distinguishing characteristic of Renaissance perspectival painting, so it is a part of Machiavelli's strategic perspective in *The Prince.* Just as we can only appreciate the artist's act of aesthetic manipulation if we consider how the painting plays upon the observer's sensory impressions and "tactile values" (Berenson, 1909, p. 11), so we can confirm Machiavelli's act of political manipulation only if we consider how *The Prince* as text plays upon his reader's—Lorenzo de Medici's—values, desires, and sense of political possibility. Machiavelli will entice Lorenzo with a vision that will overwhelm his every other thought and distort his senses. With this in mind, we might recall the moment in *Mandragola* where Machiavelli (1957, p. 25) offers a poetic comment on Ligurio's power and on the outrageous gullibility of old Nicia:

> Our doctor here, would not suspect a lie
> If he were told that jackasses can fly;
> He has a heart so set on fatherhood,
> That he's forgotten every other good.

Ligurio can deceive Nicia because of his strategic perspective. He knows how to play upon the old man's desires and to organize his field of choices by advancing some alternatives and concealing others. He makes the prospect of Nicia's fathering a child so palpable that this promise of living on comes to control Nicia's world, his sensibilities, and his every perception. Thus the old doctor is trapped; duped not only by Ligurio's wiles but by his own vanity, his grandiose expectations set the stage for his becoming the cuckold.

If we bring our own strategic perspective to *The Prince* and look to the relationship Machiavelli establishes between his text and its intended reader, we might see a political version of Ligurian deception at work. Machiavelli offers Lorenzo the promise of a different sort of fatherhood—the fathering of the state of Italy. Machiavelli's strategy is Ligurian—to promise greatness to the Medici lord and thus render him susceptible to the further flatteries that will, in fact, undo him. Nowhere in *The Prince* is the Ligurian strategy as evident as in the famous chapter 26. There Machiavelli paints the prospect of the prince as saviour of Italy, leader of his people, unifier of the fractious city-states, forever immortalized by his power and glory. The passion and spirit of these Machiavellian declamations have long troubled many of his interpreters, who puzzle over the marked contrast between this final chapter and the cold calculation of the rest of *The Prince.* However, the troublesomeness of chapter 26 begins to recede if we remember that technical precision and vivid imagination were no strangers to the Renaissance painter nor, for that matter to Ligurio as he set out to trap his prey. Accordingly, we might read Machiavelli's final call to action as the "bait," or, to return once more to perspectival art, as the "vista" he offers Lorenzo. If the chapter does its work, Lorenzo, like Nicia, will "forget every other good" and so become not only Machiavelli's puppet, but the dupe of his own grandiose expectations of earthly power and political immortality.

Thus, Machiavelli sets out to manipulate the dimensions of Lorenzo's world. After presenting a particular scope of possibilities in *The Prince*, he artfully narrows the field of choices so that, in the end, the prince will live, act, and arm himself in the manner that his advisor recommends. Thus Machiavelli performs a feat of crafty assault, even as he accomplishes an act of political veracity. Lorenzo will be lured into following his advisor's dangerous counsel even as he reads this indeed unprecedented work of *realpolitik*. The beauty of the deception, were it to work, lies in Lorenzo's belief that his acts follow from his own *virtù* and seem perfectly in keeping with his aim to maintain power in life and achieve glory after death, while in reality they work to restore the republic.

Postscript

As a text, *The Prince* succeeded in securing Machiavelli's future fame and in sealing his notoriety. As a trap, it secured nothing. From all we know, Lorenzo never even read it. The republic Machiavelli wanted so passionately to see revived in Florence did indeed come later, but by different means than he envisioned and for but three short years, after which the Medici were installed in the city again. How could Machiavelli's stratagem have come to naught? How could his trap have failed to spring as he had hoped?

The deceiver himself would have a wry and ready answer: *Fortuna*, that mysterious goddess who governs half our actions, thwarted his plans and fouled his chances for success. As the chronicler tells it, on the day Machiavelli presented *The Prince* at the palace, Lorenzo was also given a gift of greyhounds, an unfortunate circumstance indeed, for the Medici lord was more intrigued with his hounds than with princely governance (Barincou, 1961, pp. 76-78). Yet there is more to the turn of fortune's wheel than this. Despite the greyhounds, it is hard to imagine why Lorenzo, a suspicious prince, would have taken this former republican, this *mannerino* of Soderini, into his confidence in the first place. Machiavelli's every attempt to appear to be other than he was in the end was no match for his unblemished reputation as a Florentine republican, and so *The Prince* remained unread and Machiavelli unsummoned, forced to return to the countryside, where he divided his days between the "ancient courts" and his favorite and altogether appropriate pastime—snaring thrushes with his bare hands (Ridolfi, 1963, p. 140).

His misfortune takes yet one more turn, however, and the story is well known. When the republic was restored in 1527, Machiavelli eagerly reapplied for his old job at the Second Chancery. The new republicans, however, were suspicious—at the least they viewed him as an untrustworthy opportunist, at the most as a pro-Medicean. They had, it seems, been more successfully duped by *The Prince* than Lorenzo himself, and they too refused to allow Machiavelli's return to political life. The irony is hard to miss: Machiavelli had, quite simply, outfoxed himself.

Whatever misfortune was dealt this master of deception, however, his designs in *The Prince* now seem clear. We need only to remember circumstances and recall what the chronicler reports on the occasion when the advice book was eclipsed by the hounds. Upon leaving the

Medici palace, Machiavelli was said to have muttered that "though he was not a man to plot against princes, his little book would avenge him" (Barincou, 1961, p. 78). Far from avenging him, *The Prince* has for five centuries accused this Florentine patriot. His vindication is long overdue.

Notes

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1. In international relations, see Waltz (1954) and Gulick (1955); in comparative politics, see Bluhm (1965); in organization theory, see Pfeffer (1981); in political psychology, see Nardulli, Flemming, and Eisenstein (1984).

2. See, among others, Wolin (1960), Pocock (1975), Shumer (1979), Skinner (1981), Hulliung (1983), and Pitkin (1984).

3. Quotations from Machiavelli's works are cited in the text using the following abbreviations: *P*: *The Prince* (1950, pp. 3-98); *D*: *The Discourses* (1950, pp. 101-540).

4. In a related fashion. J.R. Hale (1961, p. 175) suggests that Machiavelli thought "regeneration can be best organized by a prince, but when he feels that civic virtue has been restored, he should retire." Yet surely Machiavelli would not think that a prince would simply "retire" after restoring civic virtue or, for that matter, be interested in restoring the only virtue Machiavelli considers "civic"—republicanism. He knows too much of political rulership and Renaissance Florence to expect that.

5. Machiavelli (*P*, p. 78) offers one exception to this armament rule: the prince who acquires a new state in addition to the old one where his troops are stationed should disarm the former. However, this exception does not apply to the Medici, who were in exile as private citizens before their return to Florence and in control of no "old" state.

6. Furthermore, circumstances did not change dramatically. A report on Lorenzo, made in 1515 by the Venetian ambassador, notes: "This Lorenzo has been made captain of the Florentines against their own laws. He has become ruler of Florence: he orders and is obeyed. . . . They used to cast lots; no longer . . . the majority of the Florentines have no taste for the power of the house of Medici" (Hale, 1977, p. 99).

7. Overall, Machiavelli's advice on fortresses is ambiguous and could be a short essay in itself. In *The Art of War*, Fabrizio never advises against fortresses; in fact, his discussion centers not upon their usefulness but upon their size, their relation to surrounding ditches, the design of portculisses, and the like. Machiavelli (1965, pp. 183-94) seems to be suggesting that fortresses are both important and necessary. In *The Discourses*, however, he appears resolutely opposed to fortresses, deeming them both "unnecessary" and "injurious" to prince and republic alike. "Good armies," he writes, will suffice instead of citadels (*D*, p. 365). The fact remains, however, that Machiavelli argues against a *fortezza* in *The Prince*, and the advice, whatever else it is, is fully in keeping with Florentine republican sensibilities.

8. Machiavelli's thesis was tested dramatically in 1534, when Alessandro de Medici not only disarmed the Florentines, but also constructed a fortress. The Fortezza da Basso secured Alessandro from his subjects, but not from his cousin Lorenzino, who assassinated him in bed in 1537. More importantly, however, it seems Machiavelli's prediction was accurate—Florentine republicanism was never restored. Hale (1968, p. 502) notes that the Fortezza da Basso was "the visible evidence that freedom was in fetters."

9. Machiavelli's dismissal seems to have been motivated by at least three factors: (1) his special friendship with Gonfalonier Soderini (he was contemptuously known as Soderini's *mannerino)*; (2) the hostility of the pro-Medicean aristocracy, who never ceased viewing Machievelli as an enemy of their class and a supporter of the republican cause; and (3) his reputation as the organizer of the Florentine civilian militia that had fought the Medici and their Spanish allies at Prato (see Gilbert, 1984, pp. 172-74; Hale, 1961, pp. 134-35; and Ridolfi, 1963, pp. 130-32).

10. Although my argument bears some similarity to that of Leo Strauss, especially as expressed in his *Persecution and the Art of Writing* (1952), the differences seem to me much more important. Strauss (1952, p. 33) excepts Machiavelli's name in his list of persecuted writers, but surely Machiavelli was politically persecuted; we have the biographical evidence, and Machiavelli himself (circumspectly) expresses as much on a number of occasions (Machiavelli, *P*, p. 4; 1961, pp. 10, 104, 143-44). I suspect that Strauss did not include the Florentine in his list because of his commitment to reading Machiavelli as a teacher of evil and his writings as "immoral and irreligious" (Strauss, 1958, p. 12), a view that often blinds him to the historical context and the political and biographical circumstances we must bring to bear upon our understanding of Machiavelli's aims and intentions.

11. A number of scholars have drawn this conclusion on the basis of their reading of *The History of Florence* as so much Machiavellian dissembling (see Gilbert, 1977, pp. 82-92; Najemy, 1982, pp. 575-76; and Skinner, 1981, pp. 84-86).

12. Ridolfi (1963, p. 203) cites an epitaph Machia-

velli wrote for Soderini that gives graphic evidence of his opinion of the man who might have saved the republic but had not the *virtù* to do so:

> The night Pier Soderini died
> His soul went to the mouth of Hell;
> And Pluto cried: Silly soul,
> Why come to Hell?
> Go to Limbo with the Infants.

13. Wolin (1960, pp. 21-22, 203-9) quite rightly characterizes similar attributes as part of the political theorist's vocation, Machiavelli's included. However, the tension between the role of "advisor" and the vocation of "theorist" is equally important, and though beyond the scope of this essay, deserves more concentrated attention.

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HUMAN DIGNITY, HUMAN RIGHTS, AND POLITICAL REGIMES

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It is often argued that internationally recognized human rights are common to all cultural traditions and adaptable to a great variety of social structures and political regimes. Such arguments confuse human rights with human dignity. All societies possess conceptions of human dignity, but the conception of human dignity underlying international human rights standards requires a particular type of "liberal" regime. This conclusion is reached through a comparison of the social structures of ideal type liberal, minimal, traditional, communist, corporatist and developmental regimes and their impact on autonomy, equality, privacy, social conflict, and the definition of societal membership.

The international human rights elaborated in the Universal Declaration of Human Rights and the International Human Rights Covenants often are held to be compatible with a great variety of political regimes. For example, Khushalani (1983, p. 404) argues that "the concept of human rights can be traced to the origin of the human race itself," and that "all the philosophies of our time" are committed to human rights. Likewise, Graefrath (1983, p. 6) argues that international human rights standards "can be adapted to any legal system" (cf. Buultjens, 1980; Gros Espiell, 1979; Marasinghe, 1984; Mojekwu, 1980; Pollis, 1982; Ruffin, 1982; Stackhouse, 1984; Wiarda, 1982). We argue, however, that international human rights standards are based on a distinctive substantive conception of human dignity. They therefore require a particular type of "liberal" regime, which may be institutionalized in various forms, but only within a relatively narrow range of variation. The authors cited above confuse human rights with human dignity.

"Human dignity" figures prominently in international human rights documents; for example, the International Human Rights Covenants proclaim that human rights "derive from the inherent dignity of the human person" (1966). Furthermore, every form of political regime implicitly reflects a particular social conception of human dignity. Nonetheless, human rights and human dignity are quite distinct notions.

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Conceptions of human dignity, in their social and political aspects, express particular understandings of the inner (moral) nature and worth of the human person and his or her proper (political) relations with society. Human rights, by contrast, are the equal and inalienable rights, in the strong sense of entitlements that ground particularly powerful claims against the state, that each person has simply as a human being. Human rights are a particular social practice that aims to realize a distinctive substantive conception of human dignity (Donnelly, 1982a). Conceptions of human dignity vary dramatically across societies, and most of these variations are incompatible with the values of equality and autonomy that underlie human rights. Most regimes—and their underlying social conceptions of human dignity—necessarily deny both the idea and the practice of human rights.

In order to examine the relations between human rights and conceptions of human dignity across a wide range of regimes, our analysis relies heavily on the use of ideal types—"the construction of certain elements of reality into a logically precise conception" (Weber, 1946, p. 59)—especially ideal-type conceptions of the human person and his or her obligations to and claims upon society and the state. We first specify the philosophical and structural connections between the "liberal" conception of human dignity and the principle and practice of human rights. Then we show how four major contemporary regime types, which we call *communitarian*, necessarily repudiate human rights because of their commitment to alternative social conceptions of human dignity.

The particular interpretation of liberalism we adopt provides the philosophical and structural basis for international human rights norms. This is not a paper on liberal theory. In another context we would argue for the authenticity of our interpretation, but here we claim only that it is a plausible, standard reading of the liberal tradition. Our subject in this article is human rights, not liberalism. Therefore, even if our definition should prove to be stipulative, the substance of our argument, which focuses on the social and political requirements of human rights, would remain largely unaffected.

We should also note that we do not join arguments about the content of lists of human rights. Instead, as is common in the human rights literature, we accept the list in the Universal Declaration of Human Rights without argument. In particular, we avoid rehashing old arguments about economic and social rights. Both on theoretical grounds (see Donnelly, 1982b; Donnelly, 1985, ch. 6; Shue, 1980, pt. 1). and in light of the nearly universal official acceptance of the Universal Declaration, we adopt the full list of rights it provides, with civil, political, economic, and social rights on an equal footing.

While these two simplifying assumptions narrow our focus, our argument remains significant and controversial. We contend that internationally recognized human rights require a liberal regime. Other types of regimes, and the conceptions of human dignity on which they rest, may be defensible on other moral or political grounds, but they will not stand up to scrutiny under the standard of human rights.

Liberalism and Human Rights: A Necessary Connection

Liberalism, Equality, and Personal Autonomy

We follow Ronald Dworkin (1977, ch. 12; 1985, ch. 8) in arguing that the heart of liberalism is expressed in the basic political right to equal concern and respect:

> Government must treat those whom it governs with concern, that is, as human beings who are capable of suffering and frustration, and with respect, that is, as human beings who are capable

> of forming and acting on intelligent conceptions of how their lives should be lived. Government must not only treat people with concern and respect, but with equal concern and respect. It must not distribute goods or opportunities unequally on the ground that some citizens are entitled to more because they are worthy of more concern. It must not constrain liberty on the ground that one citizen's conception of the good life . . . is nobler or superior to another's. (Dworkin, 1977, p. 273)

The state must treat each person as a moral and political equal—not assure each an equal share of social resources, but treat all with equal concern and respect. Inequalities in goods or opportunities that arise directly or indirectly from political decisions—and many such inequalities are easily justified within a liberal regime—must be compatible with the right to equal concern and respect.

Personal liberty, especially the liberty to choose and lead one's own life, clearly is entailed by the principle of equal respect: for the state to interfere in matters of personal morality would be to treat the life plans and values of some as superior to others. A certain amount of economic liberty is also required, at least to the extent that decisions concerning consumption, investment, and risk reflect free decisions based on personal values that arise from autonomously chosen conceptions of the good life. Liberty alone, however, cannot serve as the overriding value of social life, as the end to be maximized by political association.

Liberty readily degenerates into license and social atomization unless checked by a fairly expansive, positive conception of the persons in relation to whom it is exercised. If liberty is to foster dignity, it must be exercised within the constraints of the principle of equal concern and respect. In fact, autonomy and equality are less a pair of guiding principles than different manifestations of the central liberal commitment to the equal worth and dignity of each and every person.

Each human being is of equal moral worth individually, whatever his or her social utility. Individuals—regardless of who they are or where they stand—have an inherent dignity and moral worth that the state must not merely passively respect, but for which it must demonstrate an active concern. Furthermore, everyone is *entitled* to this equal concern and respect. Minimum standards of political treatment are embodied in (human) rights; they are not merely desirable goals of social policy. This implies a particular conception of the relation of the individual to the community and the state.

Man is a social animal; human potential, and even personal individuality, can be developed and expressed only in a social context. Society requires the discharge of certain political functions, and large-scale political organization requires the state. However, the state—especially the modern state—also presents particularly serious threats to human dignity. The state is easily turned to the denial of equal concern and respect, through the enforcement of a particular vision of the good life or the entrenchment of privileged inequality. Therefore, human rights have a special reference to the state, in order to keep it an instrument to realize, rather than undermine, equal concern and respect. In the inevitable conflicts between the individual and the state, the liberal gives prima facie priority, in the areas protected by human rights, to the individual.

For the liberal, the individual is not merely separable from the community and social roles, but specially valued precisely as a distinctive, discrete individual—which is why each person must be treated with equal concern and respect. The state and society are conceived, in more or less contractarian terms, as forms of association for the fuller unfolding of human potential, through the exercise and enjoyment of human rights. Human dignity, for the liberal, is largely encompassed in the vision of life as an equal and autonomous member of society, enjoying a full range of human rights.

This liberal view of man is rooted in structural changes that began to emerge in late medieval and early modern Europe, gained particular force in the eighteenth and nineteenth centuries, and today are increasingly the norm throughout the world. The "creation" of the private individual separate from society is closely linked to the rise of a new, more complex division of labor, the resulting changes in class structure (particularly the rise and then dominance of the bourgeoisie), and a new vision of the individual's relationship to God, society, and the state.

These developments are well known, and need not be recounted here. For our purposes, though, it is important to stress that in the social changes of modernization—especially migration, urbanization, and technological development—the all-encompassing moral whole of traditional or feudal society was replaced by a much more segmented social order. In particular, politics was separated from religion, the economy, and law, which were likewise separated from one another. Individuals, too, were separated from society as a whole; no longer could persons be reduced to their roles, to parts of the community. With separate individuals, possessing special worth and dignity precisely as individuals, the basis for human rights was established.

Occurring parallel to these changes in society was the equally well-known development of the modern state. The new bourgeois class was initially a principal backer of the newly rising princes and kings and their states; both shared an interest in freeing themselves from the constraints of the old feudal order. However, as the modern state's power grew, it increasingly threatened the individual citizen. Bourgeois "freemen" began to demand, therefore, that they indeed be free. Such demands eventually took the form of arguments for the universal natural rights and equality of all men. In the new socially mobile society, in which entrance to and exit from the bourgeois class was relatively unpredictable, a new set of privileges could not readily be reserved for a new elite defined by birth or some similar characteristic. Rather, in order that some (the bourgeoisie) might be able to exercise these new rights, they had to be guaranteed for all.

Thus, human rights came to be articulated primarily as claims of any individual against the state. Human rights lay down the basic form of the relationship between the (new, modern) individual and the (new, modern) state, a relationship based on the prima facie priority of the individual over the state in those areas protected by human rights. Human rights are viewed as (morally) prior to and above society and the state, and under the control of individuals, who hold them and may exercise them against the state in extreme cases. This reflects not only the equality of all individuals, but also their autonomy—their right to have and pursue interests and goals different from those of the state or its rulers. In the areas and endeavors protected by human rights, the individual is "king," or rather, an equal and autonomous person entitled to equal concern and respect.

In practice, of course, these values and structural changes remain incompletely realized even today, and for most of the modern era they have been restricted to a small segment of the population. Nonetheless, the ideal was established and its implementation begun. Even if the demand for human rights began as a tactic of the bourgeoisie to protect its own class interests, the logic of universal, inalienable personal rights has long since broken free of these origins.

Furthermore, while these processes of sociopolitical individuation and state building were first played out in Europe, they are increasingly the rule throughout the world. As a result, the structural basis for a society of equal and autonomous individuals is being universalized. Social structure today increasingly parallels the near universal diffusion of the idea of human rights and the philosophical claim that human rights are universal. Individual human rights, therefore, increasingly appear not merely as moral ideals, but as both objectively and subjectively necessary to protect and realize human dignity (cf. Howard, 1986).

Liberalism and International Human Rights

The standard list of human rights in the Universal Declaration of Human Rights can be easily derived from the liberal conception of the individual and the state. Other lists, of course, have been and may be derived from these principles, but we would argue that the near perfect fit between liberalism and the Universal Declaration reflects a deep and essential theoretical connection.

To be treated with concern and respect, whether equal or unequal, requires prior recognition of the individual as a moral and legal person, which in turn requires certain basic personal rights. Rights to recognition before the law and to nationality (Universal Declaration, Articles 6, 15) are prerequisites to political treatment as a person. In a somewhat different vein, the right to life, as well as rights to protection against slavery, torture, and other inhuman or degrading treatment (Articles 3, 4, 5) are essential to recognition and respect as a person.

Rights such as freedoms of speech, conscience, religion, and association (Articles 18, 19) protect a sphere of personal autonomy. The right to privacy (Article 12) even more explicitly aims to guarantee the capacity to realize personal visions of a life worthy of a human being. Personal autonomy also requires economic and social rights, such as the right to education (Article 26), which provides the intellectual resources for informed autonomous choices and the skills needed to act on them, and the right to participate in the cultural life of the community (Article 27), which recognizes the social and cultural dimensions of personal development. In its political dimension, equal respect also implies democratic control of the state, and thus rights to vote and to freedoms of (political) speech, press, assembly, and association (Articles 19, 20, 21).

The principle of equal concern and respect also requires the government to intervene to reduce social and economic inequalities that deny equal personal worth. The state must actively intervene to protect those who, as a result of natural or voluntary membership in an unpopular group, are subject to social, political, or economic discrimination that limits their access to a fair share of social resources or opportunities. Rights such as equal protection of the laws and protection against discrimination on such bases as race, color, sex, language, religion, opinion, origin, property, birth, or status (Articles 2, 7) are essential to assure that all people are treated as fully and equally human.

In the economic sphere, the traditional liberal attachment to the market is not accidental: quite aside from its economic efficiency, the market places minimal restraints on economic liberty, and thus maximizes personal autonomy. However, market distribution of resources can have grossly unequal outcomes. Inequality per se is not objectionable to the liberal, but the principle of equal concern and respect does imply a floor of basic economic welfare; degrading inequalities (Shue, 1980, pp. 119-23) cannot be permitted. The state also has an appropriate interest in redressing market-generated inequalities because a "free market" system of distributing resources is actively backed by the state, which protects and enforces property rights.

Differential market rewards are not neutral; they reward morally equal individuals unequally. Market distributions may be substantially affected by such morally irrelevant factors as race, sex, class, or religion, while many of the "talents" richly rewarded by the market are of dubious moral significance. Even "achieved" inequalities, should they threaten the (moral) equality or autonomy of other citizens, present at least a prima facie case for state intervention. The principle of equal concern and respect requires the state to act positively to cancel unjustifiable market inequalities, at least to the point that all are assured a minimum share of resources through the implementation of social and economic rights. In human rights terms, this implies, for example, rights to food, health care, and social insurance (Articles 22, 25).

Efforts to alleviate degrading or disrespectful misery and deprivation do not exhaust the scope of the economic demands of the principle of equal concern and respect. The right to work (Article 23), which is essentially a right to economic participation, is of especially great importance. It has considerable intrinsic value: work is essential to a life of dignity, insofar as man is conceived as *homo faber*. It also has great instrumental value, both for the satisfaction of basic material needs and for providing a secure and dignified economic foundation from which to pursue personal values and objectives. A (limited) right to property (Article 17) can be justified in similar terms.

Finally, the special threat to personal autonomy and equality presented by the modern state requires a set of legal rights such as the presumption of innocence and rights to due process, fair and public hearings before an independent tribunal, and protection from arbitrary arrest, detention, or exile (Articles 8-11). More broadly, the special threat to dignity posed by the state is reflected in the fact that all human rights are held particularly against the state. Moreover, they hold against all types of states, democratic as much as any other: if one's government treats one as less than fully human, it matters little, if at all, how that government came to power. The individual of course has social duties (Article 29), but the discharge of social obligations is not a precondition for having or exercising human rights.

We have thus moved from the liberal principle of equal concern and respect to the full list of human rights in the Universal Declaration. These rights, in turn, demand—and if implemented would play a crucial role in creating—a liberal society, and the ideal person envisioned by liberalism (cf. Donnelly, 1985, ch. 3). It would be equally simple to work back from the Universal Declaration to the principle of equal concern and respect. In fact, the association between liberalism and human rights runs so deep that the realization of human rights is the principal liberal standard for evaluating the achievements, and even the legitimacy, of any regime (cf. Donnelly, 1985, pp. 69-73).

Liberalism vs. the Minimal State

In practice, obviously, even the best of actual liberal regimes fall short of the ideal we have been discussing, and the human rights records of many self-professed liberal societies merit severe criticism. Furthermore, many avowed liberals view liberty and equality as largely antagonistic principles to be traded off against one another, rather than as complementary dimensions of the single principle of equal concern and respect. One way to make this tradeoff is to choose liberty and disregard equality, establishing a "minimal" or "nightwatchman" regime.

Advocates of the minimal state (e.g., Nozick, 1974) would largely limit the state to protecting public order and private property. To assure the good behavior of the nightwatchman, "negative" civil and political rights are also required, especially civil liberties, narrowly conceived as rights to public noninterference in the private lives (very broadly understood) of individuals. Yet minimalism also explicitly protects property rights while rejecting economic and social human rights. Beyond minimalism's obvious incompatibility with international human rights standards (which minimalists readily allow), its deep commitment to protecting private property while denying all other economic and social rights borders on logical contradiction; we can see no way that precisely and only this one economic right can justifiably be allowed on the minimalist's list of human rights.

The standard rights-based (i.e., not merely utilitarian) arguments for the right to private property in such contexts rest on the importance of guaranteed private economic activity and resources for the enjoyment of personal autonomy. Clearly, however, such an argument does not justify a right to unlimited individual accumulation: at a certain point, additional economic resources contribute nothing at all to personal autonomy, and long before that point the marginal return becomes vanishingly small. Even more importantly, exactly the same argument can be made for other social and economic rights. In fact, a substantially stronger case can be made for rights to work, a minimum standard of living, and health care.

In any case, the minimal state is almost certain to be self-destructing if it recognizes equal, universal civil *and* political rights. The denial of political participation usually rests on a desire to protect social and economic privilege, while those previously excluded from political participation tend to use their newly acquired power to obtain a fair, or at least a tolerable, share of social resources (cf. Goldstein, 1983). The emergence of the Western welfare state and popular pressure throughout the Third World for social services clearly suggests that implementing equal, universal political rights will transform a minimal regime.

The only way to avoid this would be to entrench a right to private property against the exercise of all other human rights. This is obviously unjustifiable; no plausible theory of human nature or dignity yields this one right as superior to all other human rights. However, lesser entrenchment, allowing redistribution beyond a certain level of accumulation, would be ineffective. If the point beyond which redistribution would take place were set democratically, a minimal regime would almost certainly be democratically abolished, or at least dismantled over time. Any other way of setting the limit, however, would deny the equality of political rights.

In other words, the minimal state, in its very essence, is a violator of human rights, even within the limits of its own terms of reference. Liberalism's dual pursuit of autonomy and equality is replaced in minimalism by a single-minded pursuit of autonomy understood largely as the social guarantee of the broadest possible sphere of private action, virtually irrespective of its social consequences. For the minimalist, human dignity is expressed principally in the unequal, achieved consequences of private, largely conflictual, action.

The minimal state thus is not the pure form of liberalism it is often represented to be by both minimalists and various leftist critics of liberalism. Rather, it is a perverse and internally inconsistent narrowing of liberalism that is also inconsistent with international human rights standards.

Equality, Autonomy and Communitarian Societies

Having shown that human rights and liberal regimes are closely matched, it remains to be shown that other types of regimes are incompatible with the demands of human rights, a task that we have begun with the discussion above of the minimal state. In this section we examine four major types of communitarian society, which together encompass the vast majority of contemporary nonliberal regimes.

We define *communitarian societies* as those that give ideological and practical priority to the community (sometimes embodied in the state) over the individual. Such societies regard their members as worthy of concern and respect, but only as members of society performing prescribed roles. Their concepts of human dignity, therefore, are not rooted in the notion of human rights. Communitarian societies are antithetical to the implementation and maintenance of human rights, because they deny the autonomy of the individual, the irreducible moral equality of all individuals, and the possibility of conflict between the community's interests and the legitimate interests of any individual.

Traditional Societies

Traditional societies are communal, status-based societies, governed according to principles and practices held to be fixed by tradition. They are usually ethnically homogeneous and agricultural, and frequently stateless. In traditional society, one's worth, rights, and responsibilities arise from and remain tied to differential membership in a particular society, with unequal, status-based privileges and duties resting on age, sex, caste, or other ascriptive hierarchies. The idea that one is entitled to equal concern and respect and a wide range of inalienable personal rights simply because one is a human being is utterly foreign to traditional societies. Only certain kinds of people are defined as moral persons, that is, human beings.

Although most people in traditional societies have at least some rights and privileges, these are contingent on the proper fulfillment of social roles, rather than basic personal rights held against society. Even within the recognized social boundaries, some people may be defined as outsiders, as nonbelievers are defined in strict Islamic societies, or ethnic strangers in traditional Africa. The relationship between the individual (if he or she may be so called) and society is by definition nonconflictual; everyone's interests are incorporated into the higher value system represented by the political-religious-legal decision makers. Man and society are assumed to be inseparable. The very idea of inalienable individual rights held equally by all against the community is, if comprehensible, likely to be viewed with horror (cf. Legesse, 1980, p. 124).

In traditional societies, there is no notion of the autonomous individual. One's worth, even one's existence, is defined by one's place, one's role in the community; apart from the community, one does not exist, or at least such an existence is largely without moral value. One's dignity—which usually is conceived primarily as an attribute of one's kinship, age, sex, or occupational group—is obtained or validated by discharging the (traditionally defined) duties of one's station, rather than by autonomously creating or unfolding a unique individual existence. In traditional society, there are neither human beings, in the relevant moral sense, nor equal, inalienable, and universal rights.

Many traditional societies were slave or caste societies; few were subject to democratic control in even a very loose sense of that term. Individual deviations from communal norms usually were harshly repressed, and women and outsiders usually were treated as inferior beings. Nonetheless, in theory at least, and often in practice as well, a certain sort of dignity, tied to the proper fulfillment of social roles, could be achieved by most people. Furthermore, social responsibilities usually were coupled with reciprocal social and economic protections. In traditional societies most people had a defined, secure, and (within its own terms) dignified place in society.

Therefore, one can at least understand, and perhaps even contextually justify, traditional society's denial of human rights. One might even argue that the traditional conception of human dignity is superior to that of liberalism. For example, it might be argued that most people prefer regulated, secure social roles, with their concomitant sense of belonging, to autonomy and its attendant insecurities. Insider "individuals" may well have fared better in many ways as part of traditional society than as, say, a textile worker in mid-nineteenth century England or South Korea today.

Such arguments, however, are not human rights arguments. To defend traditional society is to reject a society based on equal, inalienable, universal personal rights in favor of a status-based society. To prefer traditional society to liberalism is to reject a society of equal and autonomous individuals with inalienable personal rights in favor of a society of unequal, regulated occupants of social roles, incorporated into the community. Traditional society and human rights cannot be combined without violence to both (cf. Donnelly, 1982a and Howard, 1986, ch. 2).

Communism

By *communism* we mean an ideal type regime modeled on the structure and official ideology of contemporary Soviet bloc countries. The key feature of such societies is a communist party-state committed to total, revolutionary transformation of social and personal life. The connection between such regimes and the writings of Marx or the "authentic" Marxist tradition is an issue that cannot, and certainly need not, be addressed here. Our concern instead is with the frequently encountered argument that communist regimes are entirely consistent with international human rights norms.

While there are striking similarities between traditional and communist societies, especially in the submergence of the individual to the community (state) and in the use of social (class) roles to define individual worth, there are no less striking differences. Traditional societies have at most a rudimentary state apparatus, whereas in communist societies the state is the central social institution, despite ideologically obligatory references to its withering away. Rather than the often more or less face-to-face relations of small traditional society, communist societies range in size from millions to over a billion people. Instead of a relatively simple division of labor, they have a complex industrial division of labor, and rather than an ethnically homogeneous community, often bound together by real or mythic kinship ties, most communist countries are multi-ethnic.

This alters the entire texture of social relations. The communist state simply cannot be the functional equivalent of the traditional community; it necessarily appears as a distant, separate institution, incapable of providing the social and psychological support of close-knit traditional communities. Therefore, being subsumed into the "community" is quite a different process in communist societies.

While in traditional society the individual is never fully differentiated from the group, in communist societies individuals have been thoroughly differentiated. The modern economy, with its complex division of labor and extensive role-segmentation, necessarily produces economically, and thus socially, distinct individuals, and state bureaucracies are structured to deal (only) with (anonymous or interchangeable) individuals.

The political task for communist regimes, therefore, is to reabsorb the individual into the (state) society. The primacy of the state/society therefore must be politically created—as is underscored by the very project of revolutionary social transformation. One must be *made* a part of the community in communist societies.

However, since the basic socioeconomic organization of life under communism continues to (re)produce differentiated individuals, the process of (re)incorporation must be constantly repeated. Undifferentiated economic reincorporation is impossible, however, and ideological reincorporation, no matter how hegemonic, is insufficient. Direct political coercion, therefore, is a feature of communist collectivism that generally is absent from traditional society (because of the effectiveness of other means of social control).

As the task of the state/party/proletariat is to transform all aspects of social existence, private life is not merely subject to public regulation, but must be made public, and regulated by the state, if the revolution is to succeed. Those who follow a bourgeois or otherwise reactionary road are entitled to neither respect nor concern; at best they are ignored, and more often they are actively repressed. As one East German scholar states, "there is no freedom for enemies of the people" (Klenner, 1984, p. 15), who are defined as social outsiders. Such a belief readily leads to the identification and repression of pariah social classes and, in extreme cases, class-based "genocide" directed against kulaks or similar class enemies (Kuper, 1981, pp. 99-100).

The ethnic homogeneity of traditional society is replaced by class homogenization. "Class position," however, means simply conformity to behavioral norms specified by the state. Equality, rather than a fundamental and inviolable moral fact, is reduced to mere social sameness. In communist societies, one is equal not by birth or by nature, but only to the extent that one is essentially indistinguishable from one's fellow communist citizens, an embodiment of the new communist man. Communist societies thus produce a distinctive sort of homogenized, de-individualized person.

Communist societies obviously must violate a wide range of civil and political rights during the revolutionary transition, and necessarily, not merely as a matter of unfortunate excesses in practice. Even after communism is achieved, the denial of civil and political rights remains necessary to preserve the achievements of the revolution. The permanent denial of civil and political rights is required by the commitment to build society according to a particular substantive vision, for the exercise of personal autonomy and civil and political rights is almost certain to undermine that vision.

Furthermore, communist regimes, for all their achievements in providing economic and social goods and services, are fundamentally incompatible with economic and social *rights.* In communist societies, the possession and enjoyment of all rights are contingent on the discharge of social duties. For example, Article 59 of the Soviet Constitution (1977) states that "the exercise of rights and liberties is inseparable from the performance by citizens of their duties" (cf. Burlatsky, 1982; Egorov, 1979, p. 39). Thus, for example, access to higher education and desirable jobs is closely linked to political connections or behavior. Few rights of any sort are secure in such a regime, and no human rights, in the strong sense of equal and inalienable entitlements of all individuals, can be recognized.

It is important to stress the difference between having a human right and merely enjoying the substance of a right; between, for example, having food and having a right to food, or speaking freely and enjoying a right to free speech. In communist (and other communitarian) societies, one may be guaranteed the *substance* of certain human rights (Shue, 1980, pp. 75-76)—that is, goods, services, and opportunities may be enjoyed. They are not, however, enjoyed *as rights;* those who conform receive certain benefits, but the state may freely bestow or withdraw these as it sees fit (cf. Donnelly, 1985, pp. 11-12, 52-53, 77-80). This is as true of economic and social rights as civil and political rights. One is not entitled to these benefits simply as a human being, one does not have the special control provided by possession of a right, and one's claims to enjoy these benefits do not have the force of human rights.

In communist regimes, in fact, even as a citizen one is entitled to nothing from the state: "Human rights . . . do not exist outside the state or against the state. The state is their creator" (Lopatka, 1979, p. 7; cf. Weichelt, 1979, p. 3). Rights are acquired only by the discharge of class obligations, as defined by the state (Lieberam, 1979, p. 14). Social outsiders, such as landowners or the bourgeoisie, may lose not only their former property rights, but also all other rights.

Communist society thus rests on a social utilitarianism fundamentally incompatible with human rights. The good of society, as determined by the state/party, always takes precedence over all else. Because individual "rights" must always yield to social purposes, as enunciated by the state, such "rights" are worthless; no matter what the state does, it cannot be held guilty of violating them. Whatever the benefits and opportunities citizens may (contingently) receive—and they are undeniably substantial in some communist regimes—communism represents a thorough denial of human rights.

Corporatism

Corporatism, a principal form of contemporary right-wing regimes, can be defined as

> a system of interest representation in which the constituent units are organized into a limited number of single, compulsory, non-competitive, hierarchically ordered and functionally differentiated categories, recognized or licensed (if not created) by the state and granted a deliberate representational monopoly within their respective categories in exchange for observing certain controls on their selection of leaders and articulation of demands and support. (Schmitter, 1974, pp. 93-94)

Corporatist regimes present themselves as neutral instruments to regulate and mediate the antithetical interests of labor and capital, with other groups—such as women and youth—often officially organized and incorporated into the political structure as well, further undercutting basic structural conflicts. In practice, however, class rule is unambiguously at the heart of corporatism. The essential purpose of its ideology and political structure is to prevent further class conflict and entrench the extant economic hierarchy.

The state proclaims the equal dignity of all segments of society. Meanwhile, unequal private power and property accumulate. Workers and peasants are not necessarily excluded from a share of social benefits—for example, state controlled trade unions may be allowed to pursue certain improvements in working conditions or living standards, so long as class conflicts are denied—but they benefit only inadvertently or as a side-payment to co-opt potential opponents of the ruling corporate coalition. Equal concern and respect is at best ignored.

One variant of corporatism, which can be called *authoritarianism* for want of a better term, preserves an important sphere of private autonomy and activity. Religion and education, for example, may be left as a private matter. This privacy, however, is only a realm of public indifference. It is quite different from positive respect for or protection of a right to privacy and related human rights. Privacy (of thought, religion, belief) is not so much protected in authoritarian regimes as it is ignored—and it is ignored only as long as it does not interfere with the basic corporatist bargain. Personal autonomy is, at best, not a right but a contingent benefit.

The *fascist* variant of corporatism, however, is actively hostile to the private. In reaction to what it views as the unabated individualism of liberal society, fascism proclaims a romantic ideology of (mythic) consensus, homogeneity, and personal comfort in conforming to social roles. Human dignity is to be achieved through integration into an all-encompassing moral order, represented by the fascist state. Much as in communism, any challenge to this order, including deviation in personal values and beliefs, is treated as a threat to the entire social fabric.

This ideology of the primacy of the state readily leads to terror and scapegoating. Nonviolent denials of civil and political right are likely to be inadequate to prevent independent "political" activity, now redefined to include much of "private" personal life. Direct terror is likely to be necessary; so also is the creation and persecution of outsider or scapegoat groups, in extreme cases culminating in genocide. Such persecution not only allows the state to displace real social tensions arising from the corporatist character of society, but, in the very denial of the rights of the scapegoat group, reaffirms the unity of the fascist individual, society, and state.

From a human rights perspective, however, fascism is merely the extreme form of corporatism; fascism may actively violate more rights, but authoritarian corporatism is unlikely to protect many more. One cannot even assuredly say that life is preferable for the average individual in authoritarian corporatism; for example, if public indifference results in anomie, the intense feeling of belonging espoused by fascism may seem preferable, at least for insiders. In any case, authoritarian corporatism's public indifference to the bulk of society is certainly a denial of equal concern, while its denial of independent political action is incompatible with equal respect. Whatever the form, corporatism denies inherent personal dignity and equal concern and respect in the very bargain that defines the regime.

Development Dictatorship

One further type of communitarian regime, which we call *development dictatorship*, should be briefly noted. In development dictatorships, the principal resource of the ruling elite is control of the means of coercion, justified in the name of the most rapid possible economic development. Development, which has achieved an unprecedented ideological hegemony in the Third World, is easily presented as the moral equivalent of war, requiring the subordination of the individual to the state. Therefore, in the hands of repressive elites it nicely justifies a wide range of human rights violations, especially since the connection between particular violations and underlying development goals is likely to be at best very loosely defined.

Development dictatorship is distinguished from corporatism or communism in large measure by its class structure. In development dictatorships, economic class position is less the source of power than the result of control of the state. In nationalized economies, the organizational (Markovitz, 1977, ch. 6) or bureaucratic (Shivji, 1976, pt. 3) "bourgeoisie" is composed of occupants of high-level office in the military, the government, the bureaucracy, or the ruling party. A parasitic private bourgeoisie, essentially living off its economic relations with the state, may also exist. Controlled by members of these various elites, who have few resources other than coercion to maintain their power, development dictatorships frequently degenerate in cycles of coups and countercoups, or, once stabilized, evolve into some sort of corporatist regime. The regime rests lightly on top of society, rather than being a political representation of deep underlying socioeconomic forces (cf. Hyden, 1983, ch. 2).

Even where the regime's commitment to development is genuine, rather than merely a cover for elite depredations—so that there is an attempt to provide the substance of some economic and social rights—enshrining development as an overriding social objective assures that individuals and their rights will regularly be ignored. The value put on privacy tends to be low, as private goals might interfere with national development goals. The identification of "outsiders," economic saboteurs, or similar scapegoat groups is a common diversionary tactic when development plans fail. Full personal dignity is conceived largely as an abstract future good, to be realized only after success in the struggle for development. In general, individual human rights, especially rights against the state, the essential agent of development, must wait until development has been achieved. Once more, we are faced with a choice between human rights and alternative social goals based on a radically different conception of human dignity.

Communitarianism and the Impossibility of Human Rights

Whether communitarianism is forward or backward looking, it is structurally, ideologically, and philosophically incompatible with human rights. The view of human dignity found in all communitarian societies is that the individual realizes himself as part of the group by unquestioningly filling his social role or being loyal to the state. This conception of human dignity is incompatible with human rights.

At the core of this incompatibility is the denial of social value to personal autonomy and privacy. In communitarian societies the state (or traditional authorities), as the representative of society, must control family life, religion, education, and all other potentially independent aspects of life. Any institution that might influence or challenge the reigning regime and its ideology must be eradicated, or at least regulated; often one's very beliefs, and certainly all aspects of one's behavior, are treated as legitimate matters for social regulation.

When personal autonomy is thus denied—even repressed as a threat to society—moral equality must also be denied; some people—those who "fit in"—are treated as more worthy of concern or respect than others. The full range of international human rights must thereby be violated.

The rule of law and procedural due process are obviously incompatible with such regimes; pursuit of the community's substantive goals overrides "mere procedures." Due process is also rejected because it suggests that political organs representative of the full community might treat citizens unfairly, a possibility denied by the communitarian premise of the regime. Equal protection of the laws, and nondiscrimination more broadly, also are incompatible with communitarian regimes. In fact, positive discrimination against social deviants is essential to the political pursuit of unity; differences between individuals or groups (other than those that are officially sanctioned) are not to be protected—let alone valued as expressions of autonomy—but rather repressed, or at best ignored. In communitarian regimes, one is entitled to the protection of the laws and a guaranteed share of social resources and opportunities only to the extent that one fits within certain substantive, ideologically defined categories.

Political participation is similarly restricted, both in its substance and its participants. Debate over fundamental social and political aims cannot be al-

Table 1. Social Conceptions of Human Dignity: A Typology

Regime Type	Autonomy vs. Role Fulfillment	Conflict vs. Consensus	Repression of Outsiders	Valuation of Privacy	Equality vs. Hierarchy
Liberal	Autonomy	Conflict	No	High	Equality
Minimal	Autonomy	Conflict	No (?)	Very high	Hierarchy
Traditional	Roles	Consensus	Yes	Very low	Hierarchy
Communist	Roles	Consensus	Yes	Very low	Equality
Corporatist	Roles	Consensus	Yes	Low	Hierarchy
Developmental	Roles	Consensus	Yes (?)	Low	Equality

lowed, because they are already set by tradition or the reigning ideology. Likewise, politics is dominated by a small elite, chosen by ascription, restricted party membership, or other nondemocratic means—or, where the forms of democratic politics are utilized (e.g., communism), real control lies elsewhere (e.g., a vanguard party).

Many communitarian societies, however, do perform relatively well in providing the substance of economic rights. Many espouse, and some do achieve, relative equality of material circumstances and a basic floor of material security. But such economic "rights" are mere benefits, contingent on approved membership in the political community and on the performance of social duties. Citizens are not entitled to these goods and services; at most they may petition for them, not claim them as rights. Material security certainly is valuable, whether it is a right or a privilege, but such "security" is precariously insecure in the absence of human rights held against the state, since it can be taken away as easily as it is granted.

In sum, communitarian regimes fall short of the standard of human rights in all major areas. Much as liberalism is necessarily committed to protecting, implementing, and fostering the enjoyment of the full range of internationally recognized human rights, communitarian regimes necessarily violate the full range of human rights.

Human Dignity, Human Rights, and Political Regimes

We can pull together this discussion in a typology of social conceptions of human dignity. Table 1 lists the positions of our six types of regimes on five sociopolitical variables shown in the discussion above to be important to the social definition of human dignity. It is immediately apparent from the first four columns that these regimes fall into two broad classes, *individualistic* (liberal and minimal) regimes and *communitarian* (traditional, communist, corporatist, and developmental) regimes.

Not surprisingly, the first four variables are rather closely related. Society's attitude towards autonomy is especially important. A commitment to personal autonomy requires accepting a certain degree of social conflict, largely precludes enforcing the substantive models of belief and behavior that are the basis for the repression of outsiders, and leaves open a considerable realm of valued private activity. Likewise, a stress on role fulfillment implies a consensual society: roles are defined so as to produce consensus when properly performed; "outsiders" (those without approved or valued roles) are repressed; and privacy, which exists outside of predefined roles, is not socially valued.

There are, however, no less important differences within each of these two classes of regimes. Liberalism's commit-

Table 2. Social Conceptions of Human Dignity and Human Rights Performance

Regime Type	Equality or Hierarchy	Valuation of Belonging	Civil and Political Rights	Economic and Social Rights
Individualistic Regimes				
Liberal	Equality	Moderate	Yes	Yes
Minimal	Hierarchy	Very low	Yes	No
Communitarian Regimes				
Traditional	Hierarchy	Very high	No	Substance only
Communist	Equality	High	No	Substance only
Corporatist	Hierarchy	Varies	No	No (?)
Developmental	Equality	Moderate (?)	No	Substance (?)

ment to autonomy is matched by a commitment to equality; human dignity, for the liberal, requires the union of autonomy and equality. This commitment to equality further strengthens the tendency not to repress outsiders, or even to define outsider groups other than noncitizens, who are ignored rather than repressed.

In contrast, those at the bottom of the minimal state's social hierarchy are denied economic and social rights, as a result of the absence of a commitment to equality and the presence of an extremely high valuation of privacy (especially private economic activity) under minimalism. Thus, they may be seen as indirectly oppressed economic outsiders, and if the lower classes attempt to challenge this denial of economic and social rights, direct repression is likely. Furthermore, whereas liberalism merely accepts a certain amount of social conflict as an unavoidable consequence of personal autonomy, and even tempers conflict by the pursuit of social and economic equality, minimalists tend to view social conflict in no worse than neutral terms, and even as desirable competition between unequal, atomistic individuals.

There are also important differences among communitarian regimes. For example, there are considerable differences in the substantive bases used for the definition of social membership and roles. The most important differences, though, concern the valuation of equality and belonging (the obverse of privacy), as we can see in Table 2, which correlates the major determinants of social conceptions of human dignity with the human rights performance of each type of regime.

All communitarian regimes reject civil and political rights, which can be recognized only when individual autonomy is valued over role fulfillment. However, traditional and communist regimes, one hierarchical, the other egalitarian, do provide the substance of (at least some) economic and social rights (for insiders); that is, they provide goods, services, and opportunities, but without the power or control that comes with enjoying these benefits as rights. The value placed on equality then largely determines the range and distribution of these benefits. Communist regimes are committed to providing them equally, and in great and ever increasing quantity. Hierarchical traditional regimes, however, guarantee only a minimum floor for all (or at least all but chattel slaves, untouchables, and similar near-outsider groups).

Corporatist and developmental regimes—again, one hierarchical, the other egalitarian—do not generally offer even this much. The typical (authoritarian) corporatist regime protects only the interests of the ruling coalition (although fascist corporatism is likely to provide at least some economic and social benefits to all insiders—thus the questionable "no" in Table 2). Developmental regimes are ideologically committed to providing the substance of economic and social rights for all (insiders), at least in the long run, but since the social composition of such regimes usually belies and precludes the realization of this commitment, it remains of at best questionable practical significance.

This suggests that at least as important a variable as equality or hierarchy is the valuation of belonging. Traditional and communist regimes highly value belonging, and thus provide the substance of many social and economic rights to all insiders, while corporatist and developmental regimes, which do not guarantee even the substance of economic and social rights, place lower value on a sense of belonging. This conclusion is also implied by the comparison of liberal and minimal regimes. The absence of economic and social rights in minimal regimes is explained not simply by the absence of a social commitment to equality, but also by the very low valuation of belonging.

Only when autonomy, equality, and at least a moderately high value on belonging are combined—as in liberalism—do we find a commitment to economic and social *rights*, and not just their substance. Only with a commitment to personal autonomy will a regime actively protect civil and political rights. In other words, only in a liberal regime can there be a fundamental political commitment to the full range of internationally recognized human rights.

Other social systems may claim to have competing views of human rights. They do not. Rather, they rest on competing views of human dignity, all of which deny both the centrality of the individual in political society and the (human) rights of men and women to make, and have enforced, equal and inalienable civil, political, economic, and social claims on the state. Only liberalism, understood as a regime based on the political right to equal concern and respect, is a political system based on human rights.

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U.S. DEFENSE SPENDING AND THE SOVIET ESTIMATE

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There is a glaring and potentially important discontinuity surrounding discussions of U.S. defense expenditure policy making. On the one hand, a growing body of empirically-based research questions whether the U.S. reacts in any significant fashion to the military expenditures of the Soviet Union. On the other hand, defense policy makers routinely justify defense increases as a response to similar increases by the Soviet Union. The discontinuity is resolved in the context of a multistep model of the defense expenditure policy-making process that incorporates a new estimate of Soviet defense spending and mass public opinion. Once formulated, the model provides answers to the following two questions: (1) Does the U.S., insofar as defense spending is concerned, react to the military expenditures of the Soviet Union? (2) If so, what is the magnitude of the reaction? The answers indicate that not only does the U.S. react to estimated Soviet defense spending, but that the reaction is directly responsible for a very substantial portion of the post-1978 increases in U.S. military expenditures.

There is a glaring and potentially important discontinuity surrounding discussions of U.S. defense expenditure policy making. On the one hand, a growing body of empirically-based research questions whether the U.S. reacts in any significant fashion to the military expenditures of the Soviet Union. The thrust of recent findings is captured by Cusack and Ward's observation (1981, p. 448) that "[there is] . . . little compelling evidence that an 'arms race' embodies the primary dynamic underlying U.S. defense expenditures." Others who have been unable to demonstrate significant reactivity of the U.S. to the USSR include Fischer and Kamlet (1984), Majeski (1983), Domke, Eichenberg, and Kelleher (1983), Freeman (1983), Majeski and Jones (1981), Fischer and Crecine (1981), and Nincic and Cusack (1979). The primary determinants of U.S. defense expenditures are usually traced to incrementalism and domestic influences (Moll and Luebbert, 1980).

On the other hand, defense policy makers routinely justify the increases in U.S. defense spending as a response to increases in the spending of the USSR. In an address to the United Nations, President Reagan provides a clear indication of the need to estimate the defense expenditure patterns of one's adversaries: "the amount and type of military spending by a country are important . . . as a measure of its intentions, and the threat that country may pose to its neighbors" ("Text of Reagan's UN Speech on Nuclear Arms Race," *New York Times*, June 18, 1982). According to Secretary of Defense Caspar Weinberger, insofar as the United States is concerned,

> The most obvious and significant of these threats is the global challenge posed by the only nation that rivals us in military power—the Soviet Union. (Department of Defense—Annual Report for Fiscal Year 1982 [1981])

Explicitly stating the defense budgetary implications of the Soviet threat, Mr. Weinberger asserts:

> You're making a terrible mistake if you try to adjust your defense budget to food stamps, harbor dredgings and highways. It's the threat that makes the budget. *You've got to build your budget on the Russian budget.* (quoted by White, February 6, 1983, emphasis added)

U.S. defense expenditure decisions are often justified as a necessary response to the spending decisions of the Soviet Union.

The debate over the primary stimulus for U.S. defense expenditure decisions is an important issue given the recent and dramatic changes that have occurred in U.S. defense spending. In 1968, at the peak of the Vietnam War, the U.S. defense expenditure total of $77.3 billion was 45% of all federal spending and 9.5% of the gross national product (GNP). Over the next 10 years defense expenditures grew slowly to $103 billion—23% of federal spending and 5% of the GNP. In the ensuing 6 years, the U.S. defense expenditure total has more than doubled to $238 billion—28.1% of federal spending and 7% of the GNP. Substantial portions of the recent defense expenditure increases have been attributed by policy makers to the need to react to increases by the Soviet Union. If there is no empirical evidence that the U.S. reacts to the USSR, serious questions arise concerning both the justifications and the current level of U.S. spending. It is important, therefore, that careful scrutiny be given to the following questions: (1) Does the U.S., insofar as defense spending is concerned, react to the military expenditures of the Soviet Union? (2) If so, what is the magnitude of the reaction?

There are at least two reasons for the previously discussed discontinuity between the research community and policy makers. First, even though a great deal of the research has focused on incrementalism and domestic factors, it has overlooked several political factors that are likely to impinge on the defense policy-making process. Specifically, many models ignore the sequential character of the policy-making process; the importance of the relative balance of U.S.-USSR spending (especially if there is a spending gap); and the pervasive impact that public opinion has on all stages in the defense expenditure policy-making process. In short, existing models have not been completely specified, and hence all *ceteris paribus* inferences must be questioned.

A second reason for the discontinuity lies in the fact that there are substantial differences in the way in which Soviet defense spending is measured. Little is known about Soviet defense spending since it is formulated in secrecy, and since the USSR only reports a single line entry for defense expenditures each year. The previously cited research makes use of estimates of Soviet defense spending that are derived directly from reported Soviet data. The best of these estimates, that produced by the Stockholm International Peace Research Institute (SIPRI), essentially takes what the Soviet Union reports in its official documents and converts it to dollars.[1] Since the Soviet Union reported only minimal change in military expenditures during the 1972–1980 period, the SIPRI estimates remained virtually unchanged from 1972 to 1980, and have continued to the present to show that the U.S. is outspending the USSR. The relative lack of change in the SIPRI estimates of Soviet defense spending, coupled with the appearance of U.S. superiority, could be responsible for the "no reaction" finding.

U.S. policy makers have not accepted the reported Soviet defense expenditure figure at face value. As Kaufman (1985, p. 191) notes, "this figure is considered uninformative because its scope is not defined and changes in the announced figure do not correspond with changes in the observed level of military activities." To obtain an estimate of Soviet defense spending, the CIA has developed a building-block methodology to determine what it would cost in U.S. dollars to duplicate Soviet military activities.[2] In 1976, the CIA dramatically revised its estimate of USSR defense spending upward, based upon two assumptions: (1) the Soviet Union was willing to allocate 12–13% of its GNP to national defense, and (2) they [the CIA] had underestimated the costs to the Soviet Union of producing military hardware. By 1979 the CIA (1979, p. 3) concluded that increases in Soviet defense spending were so dramatic that the U.S. now lagged 45% behind the USSR. Consequently, the evidence used by the policy makers shows not only dramatic increases in the Soviet estimate, but also that the USSR is outspending the U.S. by a substantial amount.

To determine whether the U.S. does react to Soviet defense expenditures and, if so, to what extent, requires a three-pronged research program. It is first necessary to specify a model of the defense expenditure policy-making process that recognizes both the multistage character of the budgetary process and the fact that a Soviet estimate and public opinion are potentially relevant causative factors. In this vein, we offer a model based upon a synthesis of the organizational process and bureaucratic politics perspectives. Once a model is specified, a measure of Soviet defense spending that could have been used by policy makers is formulated. The model, in conjunction with the measure of Soviet defense spending, is then evaluated empirically. Upon completing these three stages, the presence and magnitude of a U.S.-Soviet reactive linkage can be investigated.

Modeling the Defense Expenditure Decision

Any investigation of a U.S.-USSR reaction must take place in the context of a correctly specified model of the defense expenditure policy-making process. In the sections that follow, two distinct perspectives on the policy-making process—*organizational process* and *bureaucratic politics*—are presented and then synthesized. The *organizational process* perspective provides a foundation for the structure of the policy-making process, while the *bureaucratic politics* perspective introduces substance. Together the synthesis offers a plausible and coherent characterization of the U.S. defense expenditure policy-making process.

Organizational Process and the Structure of the Process

Each segment of the U.S. government concerned with the defense budget can be viewed as an organization. Hence, to explain U.S. defense expenditures, it is necessary to determine how organizations make decisions. The underlying structure of the process employed is based upon Hoole (1976), Ostrom (1978), and Marra (1985), and derived from behavioral organization theory.

From this perspective, five "design" principles are used to construct a model of the defense expenditure policy-making process:

1. Organizations factor decisions into subproblems. Each organization deals with only a segment of the overall decision.
2. All other things being equal, organizations do not change. A change in behavior requires a stimulus.
3. An organization's initial cue is taken from the previous step in the policy-making process.
4. A limited set of environmental variables is monitored by the defense sector; consequently, much of the environment is ignored.
5. Each organization adopts decision rules that are simple, additive, and temporally stable.

Based upon these principles, the investigation will be carried out in terms of equations of the following form:

$$D_{it} = D_{i,t-1} + u^*; \quad (1)$$

$$(D_{it} - D_{i,t-1}) = u^*; \quad (1a)$$

$$u^* = a_{i1}(D_{i-1,t} - D_{i-1,t-1}) + v^*; \quad (2)$$

$$v^* = \Sigma a_{ij} E_{jt} + e^*; \quad (3)$$

$$e^*_{it} = \text{random behavior}; \quad (4)$$

where

D_{it} = organization i's decision at time t;
E_{jt} = environmental variables;
a_{i1} = interorganization connection;
a_{ij} = reaction coefficients for essential variables ($j \geqslant 2$).

Combining equations (1) through (4) into a single entity suggests the following general decision rule for actors in the U.S. defense expenditure policy-making process:

$$\Delta D_{it} = a_{i1} \Delta D_{i-1,t} + \Sigma a_j E_{jt} + e^*_{it}, \quad (5)$$

where

ΔD_{it} = the change in the organization's decision ($D_{it} - D_{i,t-1}$).

Equations (1) through (5) constitute a model of the steps of the U.S. defense expenditure policy-making process based upon principles of behavioral organization theory. The subscript i in each equation denotes that multiple organizations are involved in this process. These organizations factor the decision into subproblems, and hence each only deals with a segment of the overall decision problem.

Equation (1) suggests that a decision at time t should be very similar to the decision made at time $t - 1$. This equation indicates that a decision maker searches in the vicinity of the previous decision for subsequent responses on his or her part. There is a subtle but important theoretical distinction underlying equation (1a): the focus is on the *change* in behavior from one time period to the next. Previous behavior is viewed as a *base* from which decision makers deviate only slightly. Given the complexity of the behavior under examination and the cognitive limitations of decision makers, it is highly unlikely that the budget decisions of any of the organizations are rebuilt from zero each year. Instead, the implication of equation (1a) is that each organization accepts its own prior decision and focuses on changes in behavior from one time period to the next.

Equation (2) highlights the fact that organizations not only factor the problem into parts, but use an organizational decision that is temporally prior to their own decision as their base. By doing so, each organization is able to cut down on the time and organizational resources required to make its decision. This suggests that yearly changes are based in part on the behavior of the previous step in the policy-making process. The parameter, a_{i1}, provides an indication of the magnitude of this interorganizational connection.

Equation (3) states that deviations from the previous step can be attributed to a set of environmental variables. From the fourth design principle it follows that this is a finite set. The set of environmental variables varies with respect to the goals of the organization, and hence may differ from step to step. The composition of these subsets will be examined in the next section. Any remaining behavioral variation will be viewed as random—equation (4). Finally, equation (5) reflects the notion that the decision rules adopted by each organization in the defense expenditure policy-making process are simple, additive, and temporally stable. The observed changes can be viewed as a response to the previous organizational step and to factors in the external environment.

Bureaucratic Politics and the Substance of the Process

Formal models of the defense expenditure policy-making process have often ignored the bureaucratic politics perspective. According to Allison (1971, p. 173), the bureaucratic politics explanation is achieved by "displaying the game—the action-channel, the positions, the players, their preferences, and the pulling and hauling—that yielded, as a resultant, the action in question." As elaborated by Allison and Halperin (1972), the bureaucratic politics explanation, which focuses on intranational politics to explain national behavior, identifies three central questions:

1. Who plays?
2. What determines each player's stand?
3. How are players' stands aggregated to yield governmental decisions and actions?

Each question is important to an explanation of defense policy making.

To answer question (1), the present inquiry focuses on three organizational actors: the president, Congress, and the Department of Defense (DoD). These organizations are important because they make the three primary decisions that are necessary to produce a defense expenditure total: the president's budget request, the congressional appropriation, and the DoD's expenditure decision. In addition, these organizations represent the primary participants in the intranational political process. Accounting for the outcome of this political process provides the explanation for the defense expenditure decision as the resultant of three separate, though interconnected, decisions.

The answer to question (2) provides the necessary guidance to elaborate and specify the environmental variables in equation (3) for each organization, and thereby to specify completely equation (5) for each organization. The bureaucratic actors "choose in terms of no consistent set of strategic objectives, but rather according to various conceptions of national security, organizational, domestic, and personal interests" (Allison and Halperin, 1972, p. 43). Consequently, each player's stand is determined by different interests, and each organization is likely "to see quite *different faces of the issue*" (Allison and Halperin, 1972, p. 48). The face of the issue for each organization is determined by the answers organizational policy makers give to questions related to the following three dimensions of U.S. defense expenditure policy making:

a. How much can we afford?
b. How much is enough?
c. How much does the public want?

Answers to these three questions connect the policy maker's decisions to the external environment.

Insofar as question (a) is concerned, the defense expenditure decision rests squarely in the context of what has been called the Great Equation (Fischer and Kamlet, 1984):

$$\text{DEFENSE EXPENDITURES} + \text{NONDEFENSE EXPENDITURES} = \text{REVENUES} + \text{DEFICIT}$$

Each defense expenditure decision has implications for at least three other basic decisions: the level of nondefense spending, anticipated revenues, and the deficit/surplus. As Fischer and Kamlet (1984, p. 357) note, "each of the four terms . . . is politically or economically significant in its own right." Insofar as the defense/nondefense tradeoff (i.e., guns vs. butter) is concerned, the opportunity costs in each domain are intimately connected to estimated revenues and the deficit. Increased revenues and/or a surplus increase the opportunities for additional defense spending, while decreased revenues and/or a deficit decrease such opportunities.

With respect to question (b), the primary focus turns to a determination of how much spending is necessary to maintain a sufficient level of defense. Throughout the postwar period, it seems clear that the U.S. has focused on a very consistent set of foreign policy goals: a preference for action, anticommunism, and containment of the Soviet Union (Kegley and Wittkopf, 1982, p. 36; Ostrom and Job, 1986). To be able to act to repel communist aggression and/or contain the Soviet Union requires a substantial commitment to national defense. Hence, these goals provide a rationale both for monitoring and reacting to Soviet defense expenditures. Therefore, the answer to "How much is enough?" depends to a very great extent upon how much the Soviet Union is spending.

Since the president and all members of Congress are generally elected, it is possible that mass political attitudes can have an impact on defense decision making. To the extent that the public has an opinion about defense spending, the answer to question (c) will indicate how responsive each actor is to mass political attitudes. Russett (1972, p. 301) introduces two propositions concerning the impact of public opinion on defense spending. First, military expenditure decisions are affected by public opinion. Second, public opinion typically restricts military expenditure decisions. Even though this research suggests that negative public opinion is the primary opinion input, positive opinion may, in some circumstances, lead to increases in defense spending.

The fact that there are multiple actors involved who have different conceptions of how much the U.S. can afford, how much is enough, and how much the public wants suggests that the output of the defense expenditure policy-making process will be a *political resultant.* It is a *resultant* to the extent that no single organizational actor is in complete control of the final outcome. It is *political* because the individual organizations see various sides of the issues and respond differently to the three dimensions discussed above.

Turning to question (3), the organizational design principles lead to decision rules, in the form of equation (5), and thereby provide a clear means for determining how the individual organizational decisions are combined to achieve the final decision. It is possible, because of the interorganizational connection, to aggregate the three decisions to explain the year-to-year changes in U.S. military expenditures.

A Mediated Reactive Linkage Explanation

A synthesis of the two approaches provides stucture and substance to our model of defense expenditure policy making. The explanation is achieved by locating both the interorganizational and environmental connections that influence the decisions of each organization, and by highlighting the implications that flow from the sequential nature of the policy-making process.

Interorganizational Connection. The interorganizational connection for each organization's decision is hypothesized to be an important determinant of defense policy making. It is, in effect, the base from which changes from the previous behavior begin. We hypothesize that the president's budget is tied directly to an estimate of Soviet defense spending generated within the U.S. government. This connection seems plausible in light of the "lessons" of Munich and of the anti-communist/containment rhetoric that has been pervasive throughout the postwar era. Few political figures have suffered from taking an anti-Soviet stance, while many have suffered from being associated with the absence of such a stance. Furthermore, not only did agencies within the government begin to monitor and estimate Soviet defense expenditure behavior at the behest of the executive branch (Prados, 1982), but failure to react to these changes has proven politically unwise. One of the lessons of the 1960, 1980, and 1984 elections is that when the U.S. is behind or perceived to be behind the Soviet Union, it is politically risky for a candidate to fail to call for dramatic increases in the defense effort. The president has a strong incentive to base his request on the most recent change in Soviet defense spending.

As a consequence of interest in reelection, and because of the dynamics of the political process, Congress is in a difficult position. In spite of the potential value of defense spending to each member's district, there are many other priorities competing for scarce fiscal resources. To ease the burdens of decision making, we assume Congress reacts to the president's budget request. If the president is asking for a large increase, Congress will be hard-pressed to deny him at least a portion of the request, for the reasons discussed earlier. If the president asks for no change or a decrease, Congress is likely to go along with him, because he can be blamed directly for failing to increase the amount of money targeted for defense. Thus, Congress bases its decision on the president's budget request.

The Department of Defense has massive obligated and unobligated reserves that could conceivably be spent in a given year (Ostrom, 1978). It cannot, however, spend whatever it wishes. The Department of Defense is dependent upon Congress for subsequent appropriations, and hence is likely to be sensitive to what the Congress has appropriated (Ostrom, 1978, p. 948). Therefore, it is likely that the year-to-year changes in defense spending will be a function of the changing congressional appropriation. Congressional changes serve as the interorganizational base for the DoD.

The Environmental Connection. The three faces of the defense expenditure decision discussed previously—How much can we afford? How much is enough? and How much does the public want?—suggest that three distinct sets of environmental factors have the potential to affect the defense expenditure decision: international factors (X_{jt}), economic factors (Y_{kt}), and domestic/political factors (Z_{mt}). This classification provides the following amplification of equation (3):

$$\Sigma\, a_{ij} E_{it} = \Sigma\, b_{ij} X_{jt} + \Sigma\, c_{ik} Y_{kt} + \Sigma\, d_{im} Z_{mt} \tag{3a}$$

To keep the policy-making rules simple and to complete the specification of equation (3a), we have chosen to investigate two stimuli in each of the three environmental subsets.

In the international arena, it is hypothesized that the difference between the U.S. and USSR defense expenditure totals (i.e., the spending gap/surplus) and the extent to which the U.S. is involved in an ongoing use of the military will have an impact on the defense expenditure decision. In each case, there will be a clear signal from the international arena that more resources must be given to national defense.

In the economic arena, it is hypothesized that organizational decision makers monitor both the change in expected revenues and the size of the surplus or deficit. These two factors send different signals to organizational policy makers concerning the opportunities for growth in the defense budget. If revenues are increasing, it may be possible to increase defense spending without cutting nondefense spending or incurring a deficit. However, if the desired increases lead to a deficit, it will mean that nondefense spending must be cut, or that more resources will have to be allotted to servicing the debt.

In the domestic political arena, it is hypothesized that organizational decision makers pay attention to mass public attitudes toward defense. By and large, the public does not have strong opinions on defense. What causes policy makers to take notice, however, is the appearance of clear opinion in favor of increasing defense spending or in favor of reducing defense spending. If either one of these attitudes is prevalent, it is likely that the organizational actors will take notice and react to the public's sentiment.

A Sequential Policy-Making Process. The goals and interests that are important to each actor determine which environmental stimuli will be monitored by each actor. Since each actor's environmental reaction coefficients (b_{ij}, c_{ik}, d_{im} in equation 3a) reflect its interests and goals, the reaction coefficients for the six environmental variables may differ from actor to actor. Two of these differences are likely to be systematic, and hence can be anticipated in the form of "system hypotheses." First, the organizational process and bureaucratic politics perspectives suggest there will be an increasing reliance on the previous actor in the process (i.e., $a_{i1} > a_{i-1,1}$). As the policy-making process unfolds, the actors in previous steps will have incorporated their reaction to environmental stimuli; the temporally prior decision thus provides a current summary of the state of the environment as seen through the eyes of the previous organization. Second, as a result of this reliance, not only will subsequent actors in the process be less likely to react to the full range of environmental stimuli (i.e., the subset of environmental factors for actor *i* will not be larger than the subset of environmental factors for actor $i - 1$), but the impact of the environmental factors at subsequent stages will be attenuated (i.e., $a_{ij} < a_{i-1,j}$).

Substituting equation (3a) into the previous set of equations yields a complete decision rule for each organization:

$$\Delta D_{it} = a_{i1}\,\Delta D_{i-1,t} + \Sigma\, b_{ij} X_{jt} + \Sigma\, c_{ik} Y_{kt} + \Sigma\, d_{im} Z_{mt} + e^{*}_{it} \qquad (5a)$$

Given an equation of the form of equation (5a) for each organization, it is possible to provide an explanation for the changes in U.S. defense expenditures. On the basis of the development to this point, a number of implications arise concerning the possible presence and location of a linkage between the defense expenditure policy making of the United States and that of the Soviet Union. First, since the prior defense expenditure decision is taken as a base, it is necessary to look at changes in behavior to locate a reaction. Second, there are economic and political factors—in addition to USSR behavior—that may impinge on the decisions at each stage. Third, any reaction to Soviet spending will be filtered, via the interorganizational connection, through the remaining steps in the policy-making process.

The Soviet Estimate

In spite of the number of estimates

generated both in and out of government, every indication suggests that the members of the U.S. defense policy-making establishment utilize the estimates provided by the CIA.[3] As Burton (1983, p. 85) observes,

> The CIA's estimates of the dollar cost of Soviet defense activities are used in all branches of the United States government to measure the overall size and direction of Soviet defense programs in resource terms. Some go so far as to equate resource inputs with military capability, and to interpret them as direct measures of the Soviet military threat.

Others readily concede the importance of the CIA's estimate even if they disagree with its actual magnitude (cf. Holzman, 1980, 1982; Lee, 1977; Rosefielde, 1982).

Since 1964, the U.S. Arms Control and Disarmament Agency (ACDA) has regularly published a volume containing military expenditure data for most countries. Volumes prior to 1974 reported estimates of Soviet defense expenditures "adapted from national data and various estimates made by Western analysts." In 1975, ACDA reported that unnamed U.S. government agencies were the source of the published Soviet estimate. In 1976, and in each subsequent volume, the CIA was identified as the source for the ACDA estimate of Soviet military expenditures. Although we cannot state unequivocally that ACDA estimates prior to 1975 were derived from CIA estimates, the ACDA/CIA series that is used represents the best public estimate of the official estimates of USSR defense expenditures made by U.S. governmental agencies.

To ascertain whether the U.S. in fact reacts to the USSR, Soviet budget information that could have been used by U.S. policy makers at the time the decisions were made must be located. To this end, it is necessary to establish a number of criteria that a plausible Soviet estimate must satisfy.[4]

During the Senate Watergate Hearings, Howard Baker, senator from Tennessee, gained national exposure with his persistent question, "What did the president know and when did he know it?" This question is similarly pertinent to the study of U.S. defense spending. In order to test for a reaction to Soviet military expenditures one needs to use the Soviet data that satisfies certain temporal restrictions. Information having utility for decision makers must be available at the time the decisions are made. This implies consideration of three issues related to what information defense decision makers had and when they had it.

First, in terms of a relevant decision-making time horizon, defense policy makers obviously could not have used data revised after the fact. The ACDA/CIA presents revised data sets in each annual presentation. For our purposes, the important question is not whether the ACDA/CIA current or revised estimates are more accurate, but which set is most representative of the information available when the decision was made. We would contend that the contemporaneous estimate is the one more likely to have been used by defense policy makers. Therefore, all revised data is removed from consideration. In operational terms, the following procedure is employed: the original estimate of Soviet expenditures for a given year is the one that will be included in our data set. By chaining the contemporaneous ACDA/CIA estimates in this fashion, it is possible to approximate more closely the information available to U.S. defense planners at the time their decisions were being made. These constitute the best estimates of Soviet defense expenditures for the ACDA/CIA data.

Second, it is necessary to determine the relevant historical period within which U.S. decision makers have shown sensitivity to and concern with Soviet military expenditures. We contend that the time frame within which Soviet data has been considered is relatively brief, spanning the period from 1964 to the present. As Prados (1982, p. 245) observes,

> Estimates of Soviet military spending were a product of McNamara's tenure at the Pentagon when, in order to have Soviet spending data to incorporate in systems analysis studies, the secretary of defense put substantial pressure on CIA to produce such estimates.

There is no indication that the magnitude of Soviet defense spending was of great concern to anyone in the U.S. government prior to 1964.

The third temporal consideration is related to the lag time between Soviet spending decisions and the generation of the estimates of Soviet spending and their use by U.S. defense expenditure policy makers. We contend that there is, *at minimum*, a three-year lag resulting from *a two-year lag* in the Soviet estimate and the *one-year look ahead* by the president. The information lag is a result of the timing of the U.S. defense expenditure budgeting process. For example, in preparing his fiscal year 1984 defense budget, President Reagan finalized his request in the latter part of 1982, in order to present his proposal to Congress in early 1983. If information is to affect the president's fiscal year 1984 defense expenditure decision, it must be available no later than the latter part of 1982. The generation of the Soviet estimate is, by all accounts, an extremely complicated task. It stands to reason, then, that it would take a considerable amount of time following the end of the Soviet budget year (which is on an annual cycle) to come up with and justify an estimate of Soviet spending. Accepting these limitations, the most recent and available Soviet estimates would pertain to calendar year 1981. If there is to be a reaction to Soviet spending in the fiscal year 1984 budget, it must be to 1981 Soviet expenditure behavior. Data on Soviet defense spending for 1982, 1983, or 1984 was simply not available to policy makers at the time the fiscal year 1984 decision was made.

On the basis of the requirements that a Soviet estimate must satisfy, an ACDA/CIA Soviet defense expenditure series—hereafter referred to as X_{1t-3}—has been generated for USSR fiscal years 1964–1982. This series, presented in Table 1,

Table 1. The ACDA/CIA Soviet Estimate

U.S. Fiscal Year	Year Available	USSR Fiscal Year	Best ACDA/CIA[a] Estimate	Estimated ACDA/CIA Gap
1967	1966	1964	$ 40,000	$ 9,470
1968	1967	1965	40,000	5,880
1969	1968	1966	47,000	7,093
1970	1969	1967	52,000	15,357
1971	1970	1968	55,000	22,265
1972	1971	1969	60,000	17,785
1973	1972	1970	65,000	12,070
1974	1973	1971	74,000	472
1975	1974	1972	81,000	-5,924
1976	1975	1973	86,000	-12,777
1977	1976	1974	103,000	-25,450
1978	1977	1975	119,000	-34,100
1979	1978	1976	127,000	-39,109
1980	1979	1977	139,788	-44,231
1981	1980	1978	153,600	-50,558
1982	1981	1979	181,400	-66,387
1983	1982	1980	207,400	-74,560
1984	1983	1981	236,700	-80,604

[a] ACDA/CIA: U.S. Arms Control and Disarmament Agency/Central Intelligence Agency. Estimates are in millions of current U.S. dollars.

constitutes the best evidence, and as such, is a potential tool for testing the hypothesis that the United States reacts to the military expenditure behavior of the Soviet Union.

Recognition of the three-year information lag also has implications for U.S.-USSR spending comparisons. At any particular time, the best estimate of the spending gap—hereafter referred to as X_{2t-3}—is as follows:

$$X_{2t-3} = D_{3t-3} - X_{1t-3},$$

where D_{3t-3} is the U.S. defense expenditure total lagged three years. Note that in computing the gap for each fiscal year, we are using data lagged three years. As such, the estimated gap at $t - 3$ is the most recent information that the president and Congress have about the current state of the U.S.-Soviet balance. As can be seen in Table 1, the ACDA/CIA data suggests that since Soviet fiscal year 1972, the U.S. has trailed the USSR in spending and the gap between the superpowers has been increasing. Note, however, that given our previous argument, the U.S. could not have recognized and/or reacted to the 1972 gap until fiscal year 1975.

If the presence and magnitude of the perceived gap is important—and we contend that it is—the gap should play a prominent role in any explanation of defense expenditure policy making. This is not to say that the ACDA/CIA data is correct. Rather, if U.S. policy makers are making decisions on the basis of this data—whatever its accuracy—it is necessary to test a model with data that captures (and measures) these perceptions.

Empirical Evaluation

The following equations represent the operational version of equation (5a) for each of the three organizational actors, and correspond to the three decisions that we are modeling: the annual changes in the defense budget recommendations, appropriations, and expenditures made, respectively, by the president, Congress, and the Department of Defense:

$$\begin{aligned}\Delta D_{1t} = {} & a_{11}\,\Delta X_{1t-3} + b_{12}X_{2t-3} \\ & + b_{13}\,\Delta X_{3t-1} + c_{11}\,\Delta Y_{1t-1} \\ & + c_{12}Y_{2t-1} \\ & + d_{11}\times(Z_{1t-1}\times\Delta X_{1t-3}) + e_{1t} \qquad (6)\end{aligned}$$

$$\begin{aligned}\Delta D_{2t} = {} & a_{21}\,\Delta D_{1t} + b_{22}X_{2t-3} \\ & + d_{22}\times(Z_{2t-1}\times\Delta D_{1t}) + e_{2t} \qquad (7)\end{aligned}$$

$$\begin{aligned}\Delta D_{3t} = {} & a_{31}\,\Delta D_{2t} + b_{32}X_{2t-3} \\ & + d_{31}\times(Z_{1t}\times\Delta D_{2t}) + e_{3t}. \qquad (8)\end{aligned}$$

See the Appendix for the definitions, operationalizations, and sources for all variables. Note that each equation contains an interorganizational connection—for the president: change in Soviet estimate (ΔX_{1t-3}); for Congress: the change in the president's budget (ΔD_{1t}); and for the DoD: change in congressional appropriation (ΔD_{2t}). The environmental variables were selected from the previously discussed international, economic, and domestic/political environmental factors.[5] Where appropriate, the environmental variables have been lagged to reflect the information available to each actor in the defense expenditure policy-making process at the time each decision was made. Negative and positive public opinion (Z_{1t} and Z_{2t}, respectively) have been introduced into the model in an interactive fashion with the interorganizational connection, reflecting our contention that certain configurations of the domestic/political environment lead each organization to adjust the magnitude of its interorganizational connection.

To address the central questions identified at the beginning of this article (Does the U.S., insofar as defense spending is concerned, react to the military expenditures of the Soviet Union? If so, what is the magnitude of the reaction?), the parameters in equations (6) through (8) must be estimated and evaluated. Con-

Table 2. Coefficient Estimates for the Mediated Reactive Linkage Model

Variable Description	Coefficient	Standard Error	*t* ratio
President's Budget Request			
Change in Soviet estimate (ΔX_{1t-3})	.456	.255	1.79[a]
Spending gap (X_{2t-3})	−.322	.056	-5.72[b]
Change in war costs (ΔX_{3t-1})	.830	.241	3.44[b]
Change in revenues (ΔY_{1t-1})	.267	.046	5.79[b]
Estimated deficit (Y_{2t-1})	.414	.058	7.20[b]
Negative public opinion (Z_{1t-1})	.536	.171	3.14[b]
$R^2 = .926$; $SE = 3995$; $DW = 2.16$ Condition Number = 9.23			
Congressional Appropriation			
Change in presidential request (ΔD_{1t})	.558	.075	7.47[b]
Estimated spending gap (X_{2t-3})	−.117	.032	-3.67[b]
Positive public opinion (Z_{2t-1})	.188	.076	2.48[a]
$R^2 = .957$; $SE = 3016$; $DW = 1.73$ Condition Number = 3.79			
Department of Defense Expenditures			
Change in congressional appropriation (ΔD_{2t})	.717	.089	8.10[b]
Estimated spending gap (X_{2t-3})	−.101	.032	-3.21[b]
Negative public opinion (Z_{1t})	−.182	.078	-2.33[a]
$R^2 = .935$; $SE = 2550$; $DW = 1.84$ Condition Number = 4.86			

[a] Coefficient significantly different from 0.00 at the .05 level.
[b] Coefficient significantly different from 0.00 at the .01 level.

sistent and efficient estimates were obtained using a Zellner or two-stage Aitken generalized least squares (GLS) procedure (see Kmenta, 1971). The resulting estimates and summary statistics are presented in Table 2. Each of the equations accounts for a very substantial portion of the variation in the changes for the three annual defense expenditure decisions: 92.6% for the president's decision, 95.7% for the congressional decision, and 93.5% for the DoD decision. The standard errors for each equation are also relatively small: $4.0 billion for the president, $3.0 billion for the Congress, and $2.6 billion for the Department of Defense. Moreover, examination of the Durbin-Watson statistics suggests that the null hypothesis of no serial correlation can be accepted for all three equations. In terms of multicollinearity, the condition numbers for the three equations (Belsley, Kuh, and Welsch, 1980, pp. 100–104) indicate that the degree of collinearity is not a serious problem. At a minimum, the estimated model has a substantial degree of empirical support in explaining the changes in each stage of the U.S. defense expenditure process. To assess the plausibility of the explanation, it is necessary to examine each equation separately.

President's Request

The president's decision rule, given by equation (6), suggests the following observations. First, the president does appear to base his change in defense budget requests on changes in the ACDA/CIA estimates of Soviet military spending. On average, presidential defense budget requests increase by a factor nearly one-half the estimated Soviet increase. Note that the coefficient has the lowest *t*-ratio, suggesting that the president is not totally reliant on his base; instead, he monitors and reacts to many other factors as well. This decision rule appears quite stable, except during periods when there is substantial public opinion against defense spending. Under these circumstances, presidents appear to try to match estimated Soviet increases dollar-for-dollar. It is significant that the president's reliance on the base increases dramatically in the face of negative public opinion. During these periods, the change in Soviet spending provides an important cue and source of justification for the president's budget.

Second, the president is quite sensitive to the estimated spending gap between the U.S. and the USSR, attempting to reduce this gap by approximately one-third each year. When considered in concert with the base, presidents pay close attention to the ACDA/CIA perceptions of the military spending behavior of the Soviet Union. The Soviet estimate has a marked impact on presidential defense budget requests.

Third, the estimated coefficient for the change in war costs suggests that presidential defense budget requests will, *ceteris paribus*, decrease during periods of deescalation. That this coefficient is not significantly different from 1.0 suggests that war costs are simply added or subtracted off the top. This runs counter to previous findings that war has a ratchet effect on defense spending (Russett, 1972).

Fourth, presidential defense budget requests are directly proportional to the projected change in total federal revenues. This coefficient suggests that presidents have, on average, proposed a "share of the pie" for defense that is nearly 27% of the revenue increases. Note that this coefficient is very close to the actual defense share of the U.S. federal budget, and hence may reflect the attempts of recent administrations to divide revenue increases in rough proportion to their current share.

Finally, presidential defense budget requests appear to be inversely related to the estimate of the total federal deficit (Y_{2t-1} is negative when there is a deficit), indicating that these deficits act as constraints on the size of the proposed increase. While this finding is plausible, it must be interpreted with some caution. The size of the estimated federal deficits for the period covered by this analysis is relatively small by current standards, with a maximum value just under $91.6 billion. Whether this empirical relationship will remain stable in the post-sample period (where estimated deficits are at least twice this magnitude) cannot be determined at this time.

Congressional Appropriations

The estimated version of the congressional decision rule, given by equation (7), provides strong support for the hypothesis that changes in Congress's defense appropriations are tied to changes in presidential requests. The magnitude of the estimated coefficient would seem to indicate, however, that Congress has not been totally acquiescent to executive proposals, cutting them by an average of 44%. It should be kept in mind, though, that we are dealing with changes rather than levels, so that a percentage reduction in a proposed increase for defense is still an increase.

Congress is also responsive to public opinion vis-a-vis defense spending. Whereas the president adjusts his defense budget requests in response to negative public opinion, members of Congress appear to respond to positive public opinion. The logic for this shift is interesting. By imposing a cutting rule on presidential defense budget requests, Congress can, in effect, view itself as responsive to a public mood that feels too much is being spent on defense. On the other hand, if the barometer of public opinion favors increases in defense expenditures, then Congress may be inclined to grant the president more of his sought-after requests. As can be seen in Table 2, Congress has been even more receptive to executive requests during periods when public opinion has favored increasing the defense budget: the average cut is reduced to slightly more than 25%.

Congress does not disregard evidence suggesting that the U.S. is lagging militarily behind the Soviet Union. Changes in congressional defense appropriations are adjusted in response to the estimated spending gap between the U.S. and the USSR. There is empirical support for the hypothesis that Congress adjusts defense appropriations in response to the estimated spending gap, though this reaction is less than that demonstrated by the president. This result suggests that members of Congress do, in fact, monitor information pertaining to the international environment. Therefore, in addition to reacting to the president's request (based in part on the gap), Congress makes an independent assessment and adjusts its appropriation decision accordingly.

Finally, it should be noted that the operational version of the congressional decision rule excludes factors from the economic environment. We do not wish to imply that members of Congress are inattentive to such factors, but rather that they rely, to a large degree, on the president's incorporation of these concerns into his budget request. For example, the forecasts of federal revenues and deficits produced by the Congressional Budget Office (CBO) have been quite similar to those provided by the president; hence their information base has been nearly identical. Should these estimates begin to diverge sharply in the future, one might expect less congressional reliance on the president's assessment of the economy.

Department of Defense Spending

Of all the actors in the defense expenditure policy-making process, the Department of Defense is the most severely constrained in terms of its latitude to engage in spending behavior that varies from one year to the next. The principal cue for DoD expenditure changes is the change in the congressional defense appropriation. We have hypothesized that DoD expenditure changes will be very similar to these congressional changes. This parallel behavior is to be expected, given that DoD is dependent upon Congress for future appropriations. From the estimated version of the DoD decision rule, given by equation (8), it can be seen that these expenditure changes are strongly linked to changes in congressional defense appropriations—spending nearly 72% of the congressional changes.

The DoD is also responsive to the climate of public opinion. During those periods when there is a preponderance of public opinion against defense spending, the DoD reduces its defense expenditures to avoid controversy and a further backlash. Our analysis suggests that during such periods, the DoD will spend only 53% of the change in congressional appropriations.

There is also empirical support for the proposition that U.S. defense expenditure decisions are reactive to the estimated spending gap between the superpowers. It appears as though the DoD attempts to reduce this gap by an additional 10% each year. That is, the perceived gap has an influence independent of the previous actors in the policy-making process. This suggests the DoD feels free to spend more if there is an estimated spending gap between the U.S. and the USSR.

Finally, as with the congressional policy-making rule, there is no indication that economic factors influence the DoD decision. Consequently, the only economic impacts will be reflected in the interorganizational connection.

System Hypotheses

Table 2 provides support for the two "system" hypotheses developed earlier. The pattern of increasing reliance on the previous actor in the process is empirically supported. Moreover, it can be seen that the range of relevant environmental factors is greatest for the president and decreases for the Congress and the DoD. Even though the number of environmental factors decreases, the explained variance for each equation remains substantial.

From a substantive perspective, the acceptance of the two system hypotheses leads us to conclude that the decision rules for both the Congress and the Department of Defense are less complex than that for the president. These simpler formulations should not be taken as evidence that organizational or bureaucratic forces do not exist for these actors, but rather that an increased reliance on the interorganizational connection implies that a narrower set of interests combines to produce the annual changes in congressional defense appropriations and DoD expenditures. This greater reliance can also be witnessed in terms of the number of environmental variables resulting in adjustments to the base employed by each actor. The president responds to a much wider range of environmental factors than does either the Congress or the DoD. As a consequence of his role as budget initiator, the president plays a pivotal role in the defense expenditure policy-making process.

The Impact of the Soviet Estimate

Having determined that the basic specification has a substantial degree of empirical support, attention can be given to the two central questions identified at the beginning of this article. To determine the extent to which changes in the Soviet estimate and spending gap are translated into increases or decreases in U.S. defense spending, the joint implications of all three equations must be examined.

Equations (6) through (8) represent the *structural form* of the model under study and are based upon our theory of organizational/bureaucratic behavior. By successively substituting the right-hand side of equation (6) for ΔD_{1t} in equation (7), and the right-hand side of equation (7) for ΔD_{2t} in equation (8), we obtain the *reduced form* equation for the U.S. defense expenditure decision, ΔD_{3t}. Performing the suggested substitutions yields the following reduced-form equation for U.S. defense spending:

$$\Delta D_{3t} = p_{31}\,\Delta X_{1t-3} + p_{32} X_{2t-3} + p_{33}\,\Delta X_{3t-1} + p_{34}\,\Delta Y_{1t-1} + p_{35} Y_{2t-1} + v_{3t}, \tag{9}$$

where

$$p_{31} = [(a_{31} + d_{31} \times Z_{1t})(a_{21} + d_{22} \times Z_{2t-1})(a_1 + d_{11} \times Z_{1t-1})];$$

$$p_{32} = \{(a_{31} + d_{31} \times Z_{1t})[(a_{21} + d_{22} \times Z_{2t-1})b_{12} + b_{22}] + b_{32}\};$$

$$p_{33} = [(a_{31} + d_{31} \times Z_{1t})(a_{21} + d_{22} \times Z_{2t-1})\, b_{13}];$$

$$p_{34} = [(a_{31} + d_{31} \times Z_{1t})(a_{21} + d_{22} \times Z_{2t-1})\, c_{11}];$$

$$p_{35} = [(a_{31} + d_{31} \times Z_{1t})(a_{21} + d_{22} \times Z_{2t-1})\, c_{12}];$$

$$v_{3t} = \{(a_{31} + d_{31} \times Z_{1t})[(a_{21} + d_{22} \times Z_{2t-1})\, e_{1t} + e_{2t}] + e_{3t}\}.$$

The a_{i1}, b_{ij}, c_{ik} and d_{im} are the structural coefficients from equations (6) through (8). The reduced-form equation provides a summary of the way in which annual changes in U.S. defense expenditures are affected by each of the predetermined variables in the system; it incorporates all

Table 3. Estimated Reduced Form Coefficients, Expenditure Equation [Equation (9)]

Year	Change in Soviet Estimate ($t - 3$)	Estimated Spending Gap ($t - 3$)	Change in War Costs ($t - 1$)	Change in Revenue ($t - 1$)	Estimated Deficit ($t - 1$)
1968	.18	-.31	.33	.11	.17
1969	.14	-.26	.25	.08	.12
1970	.30	-.26	.25	.08	.12
1971	.30	-.26	.25	.08	.12
1972	.30	-.26	.25	.08	.12
1973	.30	-.26	.25	.08	.12
1974	.30	-.26	.25	.08	.12
1975	.30	-.26	.25	.08	.12
1976	.30	-.26	.25	.08	.12
1977	.40	-.31	.33	.11	.12
1978	.18	-.31	.33	.11	.12
1979	.18	-.31	.33	.11	.12
1980	.18	-.31	.33	.11	.12
1981	.24	-.36	.44	.14	.22
1982	.18	-.29	.33	.11	.17
1983	.30	-.26	.25	.08	.12
1984	.30	-.26	.25	.08	.12

Note: Each series of coefficients is computed using the formula for p_{3j} associated with equation (9), the defense spending reduced form equation. The values of each p_{3j} change in accordance with the changing values of Z_{1t-1}, Z_{2t-1}, and Z_{1t}. For example,

$$p_{31} = [(a_{31} + d_{31} \times Z_{1t})(a_{21} + d_{22} \times Z_{2t-1})(a_{11} + d_{11} \times Z_{1t-1})]$$

depicts the manner in which the reduced form coefficient for changes in the Soviet estimate varies in response to the changes in the three public opinion variables.

of the reaction coefficients from each of the three actors into a single equation. Since the two public opinion variables enter the model in an interactive fashion, the characterization of the reduced form is somewhat complicated. To simplify the presentation, the two public opinion variables have been included in the definition of each reduced-form coefficient.

A Mediated Reactive Linkage

Equation (9) illustrates the impact of each of the predetermined variables on changes in U.S. defense expenditures, ΔD_{3t}. The p_{3j} in equation (9) are called *impact multipliers* (see Kmenta, 1971), since they measure the immediate response of DoD policy makers to changes in all of the environmental factors. The expenditure reduced-form equation illustrates that the impact of the environmental factors is mediated both by the presence of three organizational actors *and* by public opinion. For example, the impact of perceived changes in Soviet military expenditures, denoted by p_{31}, is a multiplicative function of the interorganizational connections between the president, Congress, and the DoD, with adjustments made in response to public opinion. The mediation of the estimated gap between U.S. and USSR spending, p_{32}, is more complex, since it represents the combined effects of each actor's individual perception of this gap, as well as the interorganizational connections and public opinion.

Table 3 presents estimates of the derived reduced-form coefficients for each year in our sample. The reduced-form coefficients evidence some rather marked changes over time in response to changes in mass public opinion. As a basis for comparison, the coefficients for 1968 provide an indication of the values of each reduced-form coefficient when public opinion does not affect the policy-making process (i.e., $Z_{1t} = 0$, $Z_{1t-1} = 0$, $Z_{2t-1} = 0$). The model indicates that in 1968, the change in U.S. defense expenditures consisted of 18.3% of the change in the Soviet estimate, -31.4% of the spending gap, 33.3% of the change in war costs, 10.7% of the change in revenues, and 16.6% of the estimated deficit.

The changes in the predicted values of the reduced-form coefficients over time, as portrayed in Table 3, indicate that public opinion has altered the degree to which the five environmental variables have affected changes in U.S. defense spending. The results in Table 3 also shed light on the previously discussed research findings of Russett (1972). Insofar as his first finding is concerned, we concur: public opinion does affect defense expenditure decisions. Unlike Russett (1972), however, the present results indicate that public opinion can either increase or decrease the yearly change in defense spending. Furthermore, it is noteworthy that negative public opinion can lead to an expenditure increase. In the face of negative public opinion, the president increases his reliance on his base—the change in the Soviet estimate. Thus, if there is an increase in the Soviet estimate and if public opinion is negative, the president's budget request will increase. This, in turn, will be transmitted through the remainder of the policy-making process.

The spending reduced-form equation makes explicit the two ways in which the reactive linkage is *mediated.* On the one hand, the three steps, in conjunction with the interorganizational connection, serve to introduce the interests of all three organizations into the policy-making process. On the other hand, mass public opinion about appropriate levels of defense spending, both positive and negative, alters the process substantially.

The Year-to-Year Impact

To assess the expenditure implications of the impact multipliers, Table 4 uses the expenditure equation reduced-form coefficients to provide information concerning the relative impacts of each of these variables throughout the period of fiscal years 1968–1984. Table 4 presents the following quantity for each environmental variable in the system:

$$p_{3j} \times E_{jt}, \qquad (10)$$

where E_{ij} represents the environmental variables. These quantities represent the "contribution" of each of the five environmental variables to the predicted change in U.S. defense spending. For example, in 1984, the forecasted expenditure change of $16,680 million is the result of $8,690 million from the change in the Soviet estimate, $20,965 from the spending gap, -$1,616 million from the change in revenues, and -$11,359 million from the estimated deficit. The response to the USSR pushes the forecasted values up and the two economic factors drive it down.

From Table 4 it is also possible to assess the degree to which changes in the values of the predetermined variables have affected changes in U.S. defense spending. The impact of changes in the Soviet estimate has been uniformly positive throughout the 1968–1984 period. These changes, which have had especially large impacts in 1977, 1982, and 1983, can be traced to the joint impact of negative public opinion and large increases in the Soviet estimate. The impact of the spending gap has been varied. Prior to 1975, the U.S. was "ahead" (i.e., the spending gap was a spending surplus), and hence it reduced the change in defense spending. From 1976 on, however, the presence of a

Table 4. Single Period Impact, Historical Forecasts [Equation (10)] (in millions of current U.S. dollars)

	Change in U.S. Defense Expenditures		Contribution to Forecasted Value				
Year	Actual	Predicted	Change in Soviet Estimate $(t-3)$	Estimated Spending Gap $(t-3)$	Change in War Costs $(t-1)$	Change in Revenue $(t-1)$	Estimated Deficit $(t-1)$
1968	9,908	2,708	0	-1,847	4,191	670	-307
1969	520	-614	954	-1,845	397	884	-1,004
1970	-715	-702	1,483	-3,994	372	2,061	-623
1971	-2,598	-4,265	890	-5,791	-1,018	872	782
1972	604	-3,776	1,483	-4,626	-1,465	667	165
1973	-1,853	-1,815	1,483	-3,139	-1,043	2,328	-1,444
1974	4,327	-31	2,669	-123	-372	1,144	-3,349
1975	7,350	3,616	2,076	1,541	-1,440	3,012	-1,573
1976	2,991	5,707	1,483	3,323	0	2,321	-1,420
1977	7,666	6,716	6,756	7,993	0	1,923	-9,956
1978	7,485	11,340	2,921	10,710	0	5,605	-7,896
1979	11,971	8,276	1,460	12,283	0	4,756	-10,223
1980	17,827	13,585	2,334	13,824	0	5,417	-8,058
1981	23,256	21,447	3,368	18,069	0	6,840	-6,470
1982	26,754	28,485	5,060	19,413	0	8,945	-4,933
1983	27,634	26,908	7,711	19,393	0	5,074	-5,269
1984	16,929	16,679	8,690	20,965	0	-1,616	-11,359

spending gap has had an ever expanding impact on changes in U.S. defense expenditures. It can be seen that the impact of changing war costs has, with the exception of 1968, been rather inconsequential. Estimated revenues, however, have led to large increases from 1978 to 1983. The drop in expected revenues following the Reagan tax cut led to a substantial decline in the predicted change in U.S. defense spending. Finally, the estimated deficit has had a dampening effect on defense spending throughout the 1977–1984 period.

A comparison of the actual and predicted changes in U.S. defense expenditures in Table 4 shows that the model is capable of explaining a wide variety of behavior. Note especially, the ability of the model to "track" the increases prior to fiscal year 1984, and also to account for the rather dramatic decline in the size of the fiscal year 1984 increase. As can be seen, throughout the 1978–1984 era, the two Soviet spending variables and revenue increases have been the primary sources for the dramatic increases in U.S. spending. Furthermore, the presence of a deficit is pulling in the opposite direction. Even though our model is different from that presented by Fischer and Kamlet (1984), by displaying the impact of each of the variables over time we can also demonstrate the tension that often exists between fiscal and strategic considerations in the context of defense budgeting. Finally, it is of considerable interest to observe that the large increases in U.S. defense spending are attributable to changes in the five environmental variables, and not to changes in the structure/substance of the policy-making process. Wide variations in the changing defense expenditure time series can occur in the context of a temporally stable policy-making process.

The Changing Base

An underlying premise of the present explanation is that the previous spending

Table 5. Cumulative Impact, Historical Forecasts [Equation (11)] (in millions of current U.S. dollars)

		Contribution to Cumulative Forecast Value				
Year	Cumulative Change in U.S. Defense Expenditures	Change in Soviet Estimate ($t-3$)	Estimated Spending Gap ($t-3$)	Change in War Costs ($t-1$)	Change in Revenue ($t-1$)	Estimated Deficit ($t-1$)
1968	9,908	0	-1,847	4,191	670	-307
1969	10,428	954	-3,692	4,589	1,554	-1,311
1970	9,713	2,437	-7,686	4,961	3,615	-1,935
1971	7,115	3,326	-13,477	3,943	4,487	-1,153
1972	7,719	4,809	-18,103	2,478	5,153	-988
1973	5,866	6,292	-21,242	1,436	7,482	-2,432
1974	10,193	8,961	-21,365	1,063	8,626	-5,781
1975	17,543	11,037	-19,824	-377	11,638	-7,354
1976	20,534	12,520	-16,501	-377	13,959	-8,774
1977	28,200	19,276	-8,508	-377	15,882	-18,730
1978	35,685	22,197	2,202	-377	21,486	-26,626
1979	47,656	23,657	14,486	-377	26,242	-36,849
1980	65,483	25,991	28,377	-377	31,659	-44,908
1981	88,739	29,360	46,446	-377	38,499	-51,378
1982	115,493	34,420	65,859	-377	47,445	-56,311
1983	143,127	42,131	85,252	-377	52,519	-61,580
1984	160,056	50,821	106,216	-377	50,902	-72,940

decision is the base; each year's change becomes part of the base for the subsequent year. Table 5 shows that if changes in the U.S. defense expenditures are accumulated over time (i.e., adding all of the changes), the base has increased by $160,056 million since 1967.[6] The impact of each of the environmental variables can be accumulated in a similar fashion to obtain a rough indication of the implied source of changes in the base. For this reason, it makes sense to look at the cumulative impact of each variable on the rising level of U.S. defense spending. In this vein, Table 5 provides the following accumulation for each of the five factors:

$$\sum_{t=1968}^{1984} p_{3j} \times E_{jt}. \qquad (11)$$

These values represent the accumulated impact of each variable over time; hence, the 1984 values for each of the predetermined variables provide an indication of how much of the $160,056 million change in the base from 1967 to 1984 can be attributed to each of the environmental variables.

As can be seen in Table 5, the cumulative impact is as follows: $50,821 million for changes in the Soviet estimate, $106,216 million for the spending gap, -$377 million for changes in war costs, $50,902 million for changing revenue, and -$72,940 million for the deficit. The two measures of Soviet behavior sum to over $157,000 million, and the two economic measures sum to over -$22,000 million.[7]

It is noteworthy that the cumulative change in defense spending totaled only $35,000 million through fiscal year 1978. Since that time, the total changes have quadrupled. The primary source of the increase is the spending gap. From the initial time that the ACDA/CIA reported that the U.S. spending lagged behind that of the Soviet Union, U.S. defense expenditures—and the base—have increased dramatically. In the context of this model, it is clear that the reaction to the perceived changes in Soviet defense expenditure behavior, as well as the perceived gap, have been the primary sources of the dramatic rise in U.S. defense spending in the post-Vietnam era.

Conclusions and Implications

On the basis of the preceding analyses, five conclusions merit special attention. First, the estimated version of the model provides evidence that public opinion concerning the appropriate magnitude of the U.S. defense effort plays an important role in U.S. defense expenditure policy making. As shown in Table 3, some of the reduced form coefficients are altered considerably in the presence of certain configurations of public opinion. Furthermore, the impact of the changing coefficients can be substantial, especially in the case of negative public opinion and large increments in the Soviet estimate. Second, after indicating that a usable Soviet estimate must meet certain standards (annual production, restriction to the post-1964 period, and availability when policy makers are deliberating), the model demonstrates that changes in U.S. defense spending are directly related to perceived changes in Soviet spending, as well as to the perceived spending gap between the U.S. and USSR. Not only is there a reaction at each step in the policy-making process (see Table 2), but as Tables 4 and 5 illustrate, the two measures of Soviet behavior have provided the primary impetus for increases in the U.S. defense effort. Third, the discontinuity between the justifications of the policy makers and findings of the research community disappears when the specification of the model of the policy-making process is enhanced and a more appropriate estimate of Soviet defense spending is used. The findings from our research are consistent with the assertions of the defense policy makers: the U.S. does react to the ACDA/CIA Soviet estimate. Fourth, as a consequence of the sequential character of the policy-making process, the impact of the environment is mediated by both the interests/goals of the three organizational actors and public opinion. The changes in U.S. defense spending must be viewed as a *political resultant.* Fifth, changes induced by public opinion and/or large changes in the five environmental inputs can lead to dramatic changes in the expenditure decision within the confines of a stable policy-making process.

It is important to note that the previously discussed reactive linkage is between estimated Soviet spending and actual U.S. spending. This has two important implications for the ongoing debate about the appropriate level of defense spending. First, as noted previously, the ACDA/CIA series is constantly revised to take into account mistakes that have been made in previous estimates. As a consequence, the revised data is much more accurate than the current data (Kaufman, 1985). Even though there is a high degree of uncertainty associated with the most current estimate, our earlier arguments concerning the previous year's decision as the base imply that it is unlikely the revised data will be incorporated into defense expenditure policy making. Therefore, policy makers proceed with uncertain estimates of Soviet defense expenditures.

Second, the uncertainty concerning the ACDA/CIA's Soviet estimate may be systematically biased toward overestimating the level of Soviet spending. Recently, the CIA has reported that it overestimated increases in Soviet defense spending during the entire 1977–1983 period (Kaufman, 1985). Kaufman (1985, p. 181) notes that "the process which led to the 1983 revisions began about a year earlier. . . . To date, the agency has not disclosed . . . why it took until 1983 to disclose the fact that the trend had changed."

One reason it took so long to disclose the reduction in the Soviet estimate could be that the ACDA/CIA Soviet estimate is more than a best estimate of Soviet behavior; it could be affected by bureaucratic self-interest. To assess the veracity of the ACDA/CIA estimates of Soviet defense spending, it seems prudent to begin to consider the manner in which these estimates are formulated. Due to timing problems, we chose not to incorporate the generation of these estimates into the basic model; they are treated as exogenous inputs.[8] The organizational/bureaucratic perspective we adopt may provide future researchers with a set of initial assumptions about the behavior of the actors responsible for the production of these estimates. For example, a view of the CIA as a "player in a position" suggests they will see a "face of the issue" that is a product of their own organizational interests. Although we have no knowledge that organizational interests have an impact on the Soviet estimate, the secrecy surrounding the ACDA/CIA and the complexity of the building-block method raise the possibility that the Soviet estimate could be biased in a "strategic" fashion. There are ample precedents in other areas of bureaucrats behaving so as to maximize their self-interests.

According to our model, reactions to the changes in the Soviet estimate and attempts to close the spending gap (which may not have existed), have accounted—as can be seen in Table 5—for over $100 billion of the increase in defense spending since 1978! Therefore, not only is U.S. defense spending higher than it would have been had the lower growth estimates been used, but the changes that have already occurred have been incorporated into the base. It is unlikely that the U.S. defense expenditure total will be reduced in light of the recent CIA disclosures of overestimation. The structure of the defense expenditure policy-making process and the Soviet estimate have enormous consequences for current and future considerations of the defense/nondefense tradeoff.

Appendix. Data Definitions

The *dependent variables* are the annual changes in presidential defense budget requests (ΔD_{1t}), congressional defense appropriations (ΔD_{2t}), and expenditures by the Department of Defense (ΔD_{3t}). Data for the first two variables were taken from the *Congressional Quarterly Almanac* (1984, 98th Congress, 1st session, 1983 vol. 39). *The Budget of the United States Government* (1964–1984) is the source for U.S. military expenditures. Each of these variables is reported in current dollars on a U.S. fiscal year basis.

Changes in the Soviet estimate (ΔX_{1t-3}), as indicated above, were taken from the U.S. Arms Control and Disarmament Agency's annual publication, *World Military Expenditures and Arms Transfers* (1963–1982; 1985). These yearly changes are the ACDA/CIA estimates of Soviet military expenditures at time $t - 3$ minus the ACDA/CIA Soviet estimates at time $t - 4$.

The *estimated spending gap* (X_{2t-3}) is the difference between U.S. military expenditures at time $t - 3$ and the ACDA/CIA Soviet estimates at time $t - 3$.

Changes in U.S. war costs (ΔX_{3t-1}) are the estimated additional costs above what would have been spent to maintain a baseline force in peacetime in time t minus similar costs in time $t - 1$. These war costs were taken from *The Economics of Defense Spending: A Look at the Realities, July 1972* (DoD, 1973, p. 149). This document was produced by the U.S. Department of Defense (comptroller).

Estimates of revenue changes (ΔY_{1t-1}) and *total deficits* (Y_{2t-1}) were taken from the U.S. Department of Treasury document, *Treasury Bulletin*. Each of these variables is a "current estimate" rather than the actual total for a given fiscal year. Revenue changes reflect the forecasted revenues for fiscal year $t-1$ minus the forecasted revenues for fiscal year $t-2$. The deficit variable is the anticipated difference between forecasted revenues at time $t-1$ and total planned expenditures at time $t-1$.

Public opinion data on defense spending was taken from *Gallup Poll Reports* (March 1985, Report No. 234). The negative opinion variable (Z_{1t}) was coded 1 whenever there was a plurality of public opinion favoring decreased defense spending. This occurred in 1969, 1971, 1973-74, 1976, and 1982-83. Otherwise, this variable was coded 0. The positive opinion variable (Z_{2t}) was coded 1 whenever there was a plurality of public opinion favoring an increase in defense spending. This occurred twice, in 1980 and in 1981. Otherwise, this variable was coded 0.

Notes

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1. Prior to the 1974 volume of the *SIPRI Yearbook*, official Soviet budget figures were simply converted to dollars using a purchasing power parity rate. Beginning with the 1974 volume, however, SIPRI made adjustments to the single line item Soviet budget figure by estimating "budget residuals" in other categories that might be expected to hide military outlays, e.g., research and development totals in the science allocation in the All-Union budget. This approach is very similar to that advocated by Lee (1977). Since 1979, SIPRI has published what it terms a "compromise" figure that is somewhere between official Soviet pronouncements and CIA estimates. SIPRI's estimation procedure, like that of the CIA, appears cloaked in secrecy. Upward revisions in recent volumes reflect changes in the purchasing power parity rate. For further details, see SIPRI (1974, p. 172 ff.; 1979, pp. 28 ff.; 1985, pp. 250 ff.).

2. There is a difference between defense *costs* and defense *expenditures*. The spending estimate does not necessarily reflect the costs to the Soviet Union of its defense effort (Kaufman, 1985). With respect to the accuracy of these estimates, much of the controversy focuses on the "building block," or "direct-costing," method that the CIA employs. This method "involves defining the set of activities which make up the US defense effort, estimating the magnitude of comparable Soviet activities in physical terms, and applying US prices to the Soviet activities" (Burton, 1983, p. 86). Based upon this procedure, the CIA has recently concluded that the Soviet Union outspends the U.S. by a wide margin. Some have taken issue with the technical validity of dollar-dollar comparisons, the various weights assigned by the CIA to different components of the Soviet defense effort, the "index-number problem," etc. (Burton, 1983; Cockle, 1978). Others have taken issue with the "bottom line." Holzman (1980, 1982), in the only extended study of the index number issue in military expenditure comparisons, argues that the CIA routinely produces dollar estimates that inflate the true value of Soviet defense activities. The opposite view is offered by Lee (1977) and Rosefielde (1983). Our intent is not to resolve or even address the issues surrounding the accuracy of the CIA estimates. Instead, we acknowledge the controversy over how much the Soviet Union spends for defense, and turn our attention to the effect(s) of this estimate on the U.S. defense expenditure policy-making process.

3. Given the secrecy surrounding this estimate, determining which group(s) within the CIA is/are responsible for its production is difficult. Richelson (1985, p. 28) identifies the Office of Strategic Research, which is housed within the Directorate of Intelligence, as the production source for *A Dollar Cost Comparison of Soviet and U.S. Defense Activities, 1968-1978.*

4. The Soviet estimate is presented in a variety of different metrics, depending upon whether the analyst chooses current or constant roubles/dollars. We recognize that the choice of metric will have an impact on the magnitude of the estimate; we have chosen to characterize the estimate in terms of current dollars, since this is the metric in which the defense budget decisions are presented.

5. As a check on the specification, we estimated an equation containing all six variables for each actor, using ordinary least squares (OLS) regression. In addition, collinearity diagnostics were performed on each equation (cf. Belsley, Kuh, and Welsch, 1980) and highly collinear factors were identified. These equations proved—from both a statistical and substantive point of view—to be the best.

6. The base year for predicting levels is 1967, since our first observation on ΔD_{3t} is the change from 1967 to 1968.

7. The sum of these five factors does not equal $160,058. The difference is due to the accumulated error.

8. The timing problems arise because if one

follows the logic of our argument in operationalizing the Soviet estimate, it is necessary to explain X_{1t-3} with variables whose subscripts would be t, $t - 1$, and $t - 3$.

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THE POLITICAL ECONOMY OF THE TARIFF CYCLE

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How can protectionism and "free" trade succeed one another? Our answer focuses on the changing balance of private actors' political demands. These actors acquire interests in tariff policies because their assets are spatially concentrated, and trade in these assets is subject to various limitations. Actors in regions experiencing no new investment in an established industry ("old" regions) have interests that sometimes differ from those in regions where there is new investment. We show that old regions have no reason to be involved in tariff politics at business cycle peaks; during troughs, whether a state becomes more or less protectionist depends, ceteris paribus, *on the relative political strength of old import-competing and old exporting interests. If old import-competing industries outweigh the old exporters, then protection will tend to increase at the trough and decrease at the peak of a business cycle; the opposite result occurs when old exporters are more influential.*

Although there is an elaborate normative theory of international trade and tariffs, political scientists and economists have been less successful in developing an adequate positive theory of tariffs. One feature of tariff policy is that tariff levels tend to move in a cycle coupled with the business cycle. Tariff barriers are lowered during periods of relative prosperity, only to be raised again during world recessions. Although the existence of a "tariff cycle" coupled to the business cycle is commonly assumed by journalists, policy makers, and some scholars (Strange, 1979), the underlying logic of such a cycle and its relation to the level of economic activity have never been fully explored.

An understanding of the politics of tariff making is of course important in its own right: the regulation of international trade is a ubiquitous policy tool in capitalist states. (Indeed, commercial policy antedates fiscal and monetary policy literally by centuries.) It is also important as a means to understanding some broader questions: Under what circumstances will interest group demands be met by countervailing pressures from other interests? How can a political

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system move from liberal to nonliberal policies and back again? How is it possible for the entire international capitalist system to oscillate between periods of relatively free trade and relatively tightly managed trade?

It is natural to begin the task of constructing a theory of the politics of protection by an analysis of the effect of restrictions on foreign competition upon the distribution of domestic income. Political scientists have studied ways by which interest groups with identifiable stakes in particular protectionist measures press their demands (Bauer, Pool, and Dexter, 1963; Schattschneider, 1935). Economists have attempted to identify the structural characteristics of an industry—size distribution of firms, labor intensity, geographical dispersion—that tend to increase the industry's political leverage (see, for example, Lavergne, 1983, and the references therein). Published analyses typically examine protectionist pressures in terms of industry interests. The focus on the industry implicitly assumes that once a productive input is employed in a particular industry it is committed to that industry and cannot easily be redeployed to other industries. Therefore, the earnings of these committed inputs are tied directly to the level of demand experienced by the industry to which they are committed.

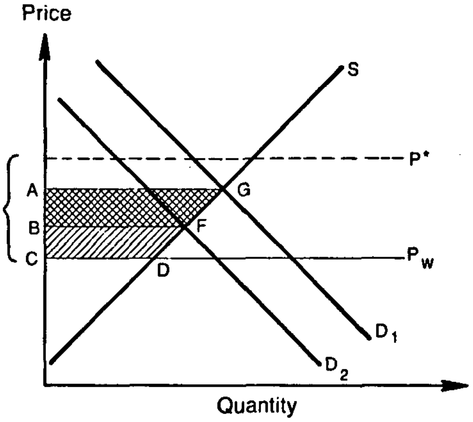
The effect of commercial policy on the distribution of income is different when factors of production are mobile than when they are not. If factors were not committed to use in particular industries, but could be easily redeployed in response to shifts in the pattern of product demand, then factor owners would not be concerned with any particular industry, but with the economy-wide demand for their particular input. Commercial policies that expanded the demand for labor relative to capital would be favored by labor; the converse would also be true. Therefore, the lines of political cleavage would fall between labor and capital on tariff politics (see, for example, Stolper and Samuelson, 1941).

If factors were mobile, we should not expect to see a high degree of correlation between the patterns of lobbying activity of labor and capital. Conversely, if resources once committed to an industry have returns dependent on that industry's level of activity, then we should see owners of both labor and capital *within a given industry* taking the same position on protectionist measures. Magee's (1978) study of lobbying activity found that owners of both labor and capital within a given industry tend to take the same position on protectionist measures. This finding supports the assumption, inherent in existing studies, about industries constituting interest groups.

While previous studies focusing on industry interests in protection have enhanced our general understanding of the politics of protection, it is apparent that something important is missing from a positive theory of tariffs based solely on the analysis of industry interests. The notion of rent seeking implies that owners of industry-specific factors should be as vigorous in their defense of existing protection as they are in seeking additional protection. Furthermore, as Figure 1 illustrates, both the losses suffered by reductions in protection and the gains secured by increases in protection are greater when such changes occur in high-demand conditions than when they occur in low-demand conditions. Therefore, if industry-specific forces in import-competing industries *alone* were to produce the temporal pattern of changes in the level of protection, we should observe barriers rising when industry demand is high and possibly eroding when industry demand is diminished. This is in direct conflict with the evidence (Gallarotti, 1985; McKeown, 1984; Takacs, 1981) that trade barriers tend to be raised when industry demand is low. Of course, it is often the case that when one industry is experiencing a state of low or high demand, many other industries are also experiencing a similar condition. Therefore, a state of demand cannot readily be identified as a factor that can account for variations in levels of protection across industries at any specific point in the business cycle. For this reason, it is best to consider theories based on industry-specific factors alone as theories which explain interindustry differences in the *average* level of protection afforded *over the course of a business cycle*, rather than an explanations of changes in the level of protection at any given time. A fully adequate theory of tariff policy should account not only for which interests receive protection, but when.

The theory we present accepts the general notion that tariffs are shaped by interest group political activities, but we conceive of interest groups in a way that departs significantly from previous treatments. We assume that some resources are not only industry specific, but also *place specific*. The introduction of place-specific resources in conjunction with consideration of the pattern of ownership of these resources allows us to identify an intertemporal asymmetry of interests among members of the same industry. In particular, if the typical individual owns a geographically diversified portfolio of assets, place specificity would add nothing to the analysis, since the gains to any one industry from rent seeking (no matter where located) will be offset by corresponding losses to others.[1] However, most individuals are not so diversified. While ownership of some assets is diversified geographically, ownership of others is not. Because the assets individuals own tend to be concentrated where they live, place specificity plays a crucial role in the theory, and allows us to account for the relation of tariff to business cycles.[2] The consideration of place-specific resources also yields a set of hypotheses on tariff coalition formation that have not heretofore been discussed.

Figure 1. An Industry's Gains from Tariffs Under Conditions of Low and High Demands



P is the prevailing world price; the tariff, t, raises the price of imports to P*. Under conditions of high demand, D_1, the rent accruing to protected industries is thus the polygon AGDC; under conditions of low demand, D_2, the rent is only BFDC.

In what follows, we first show how the economic stakes of actors change across regions and industries, and over time. We then consider factors that mediate between the economic stakes and the expression of effective political demand. Finally, we present some testable hypotheses that emerge from the theory, and consider empirical findings gleaned from other studies.

In order to make progress on this task we adopt the following strategy. First, we attempt to explain changes in the pattern of societal demands for tariffs. We do not claim to have a complete theory of tariffs per se, because we have not modeled the "supply side"—i.e., the government. Our reading of public choice literature on the supply of regulation (e.g., Peltzman, 1976) leads us to conclude that supply-side dynamics are generally complementary to those of the demand side. The political science literature on the domestic politics of international economic relations (e.g., Katzenstein, 1978) advances several considerations that are present in individual cases, but that need not be contained in a general theory. Our theory thus deliberately pursues the logic of pluralist theories, to the deliberate exclusion of alternative theoretical perspectives that assign more pride of place to state actors' motivations. Second, we assume tariff increases in one country will be met by retaliatory increases in other countries. This rules out the use of tariffs as a device to increase aggregate demand. Our theory accounts for tariff policy changes on other grounds.

Third, our analysis of nontariff protection is limited. Our theory demonstrates why the general pattern of societal demands for nontariff protection will follow that of demands for tariff protection, and also suggests a circumstance when an industry might seek nontariff protection rather than tariffs, but that is the extent of our analysis of this topic. Nontariff protection often takes the form of administrative action triggered by individual petitioners, and therefore lacks the coalitional dynamics of "pure" tariff politics. Moreover, nontariff protection can be defined very broadly, so as to encompass virtually any government policy that might yield economic benefits for a population of asset holders. In any specific historical context, the array of possible governmental actions may be quite large, and we could not hope to develop a general theory that would encompass all the political choices an interest group might face regarding the type of governmental action it should demand.

Finally, we adopt the simplest and most common assumption about the decision-making processes of actors: utility maximization. We do this because we wish to highlight as sharply as possible the importance of certain structural features of a capitalist economy in producing certain political results. While the departure of actors' actual decision processes from utility maximization may well account for some important aspects of tariff politics (McKeown, 1984), the argument we present is designed to demonstrate that structural features have a potent effect on tariff politics, independent of whatever argument we wish to make about decision processes. Our argument does not depend upon actors being able to make highly sophisticated and accurate forecasts or highly complex calculations of expected net benefits. We are thus optimistic that the argument is robust with respect to many reasonable departures from orthodox utility maximization.

Actors' Economic Stakes in Protection

Overview of the Essential Ingredients

We present a theory of international tariff levels in which spatial characteristics of industries and regional representation in the political process are central. In particular, we conceive of a nation as divided into distinct political/economic regions. Economically, each region has a particular industrial base. In order to highlight our argument, we will assume that each region possesses a single dominant industry, either import-competing or exporting. Politically, each region has (elected) representation in the national government, and each regional representative is motivated to secure regional benefits at the lowest possible cost. Voters vote their regional pocketbooks: they elect candidates who deliver (or seem to deliver) regional economic benefits.[3]

Industries tend to shift the regional concentration of their capital within a nation over time. This relocation of industrial activity is just the predictable response to shifts in interregional comparative advantage. Business location decisions are responsive to changes in demand patterns due, for example, to population movements, differing regional tax environments, various regional institutions such as unionization laws, transport cost changes, and so forth. Thus we distinguish between *old regions* and *new regions* within an industry. Note that it is not necessary to our theory that each industry be located in both an old and a new region within a single country. Some industries may have only old regions, while others may have only new ones.

We define *old regions* as those in which the dominant industry's productive capacity is contracting at some depreciation rate, even when national industry productive capacity is stable or growing. Correspondingly, *new regions* for an industry are characterized by capital accumulation. Therefore, although *total* industry net investment may be zero, *regional* industry net investment may be either negative or positive. This is a crucial asymmetry among regions that are dominated by a given industry.

Individuals are deemed to be interested in commercial policy to the extent it can be used to increase individual net wealth. Each individual owns a portfolio of assets consisting of quantities of capital and labor. The capital may have both a nonhuman and human component. The human capital, we assume, consists of industry-specific skills. A possible exception is provided by new entrants to the labor force, who are likely to possess relatively small human capital endowments and lower migration costs. Young workers are thus freer to respond to adverse labor markets by exiting a region. The nonhuman capital is of two varieties: ownership claims on various industries and place-specific assets such as land, housing, and so forth. We postulate that stock ownership has no geographic correlates, because stock is typically traded in national markets in which buyers typically diversify their portfolios. However, ownership of physical assets (homes, small businesses) is geographically concentrated. Owner-managers obviously have both types of assets, but our prototype capitalist is assumed to have a stock portfolio selected without regard to geography.[4] This is another important asymmetry in the model. Labor, as opposed to human capital, is assumed to be specific to no particular industry, but is regionally specific to the extent there are migration costs. Human capital is similarly place-specific, to the extent that skills are tied to the region's nonhuman capital, because the nonhuman capital in a given region is of a particular "vintage."

The argument proceeds as follows. First, we discuss an individual's asset portfolio and argue that it is disproportionately loaded with regionally-specific assets. Second, we focus on the effects of tariffs, and conclude that old import-competing regions benefit most from protection during cyclical troughs of the business cycle and do not benefit at all during peaks. This asymmetry is the key to our theory of the tariff cycle. Finally, we discuss the interests of other types of regions and explain how the changing economic stakes of different regions generate a tariff cycle coupled to the business cycle.

Individual Wealth and Interest in Protection

In principle, any individual's asset portfolio could be as diversified as desired. Assets could be diversified across industries, skills, and regions. In fact, impediments such as incomplete insurance markets (due to moral hazard and adverse selection problems), legal constraints on trade in human capital, certain tax laws (such as those favoring home ownership), and monitoring costs bias many asset portfolios toward a disproportionately high holding of assets specific to one's own region. For example, over 60% of

Americans own their homes, or at least the associated debt. Similarly, local businesses are regionally specific in regard to owned physical capital, reputation, and the presence of nearby customers. Furthermore, for many individuals, their potential labor services and existing human capital endowments are their most important assets. These assets are place-specific to the extent that job search and migration costs outside the region are significant or job skills are tied to uniquely place-specific capital (as, for example, with eastern underground coal miners versus western strip coal miners).[5] However, restrictions on slavery and on indenture prevent effective ownership of others' human effort, and so inhibit asset diversification.

All of these forces conspire to give individuals a special interest in the value of their particular region's assets. Thus, there is a *coincidence of spatial economic interests* among a region's residents.

Of course, individuals also own claims to the various industries' assets, both at home and abroad, and so are interested in particular industries regardless of their location. Claims to industry assets, however, may be diversified through the stock market across industries and across regions; we argue that this diversification dilutes the portfolio owner's interest in the health of any particular industry to the point where political action is not rational for them. Consequently, an individual's interest in any particular industry depends especially upon how increased industry activity translates into increased demand for regionally specific assets. For example, the Pittsburgh steel worker is less interested in the American steel industry than in its health in Pittsburgh.

Old Regions and New Regions

Any change in the economic environment typically alters the pattern of efficient production between nations. Over time the composition of a nation's exports and imports responds to such changes. The United States, for example, has evolved from an agriculturally based economy to a manufacturing and service-oriented economy. Just as importantly, changes in the economic environment induce shifts in the pattern of regional comparative advantage within a nation. In the United States, coal and ore production have moved westward. Steel production has relocated to both the South and the West. The textile industry has contracted in New England but expanded in North and South Carolina.

This relocation of production activity creates for any given industry "old" and "new" regions. In old regions industry is contracting, while in new regions it is expanding. Old regions tend to have a high proportion of the industry's capital stock, older vintages, and a larger work force; most importantly, they exhibit negative net investment. Firms are targeting new regions for new investments, but the inhabitants of these regions cannot easily determine their good fortune until some investment decisions are implemented. Since international and intranational comparative advantage shifts are occurring simultaneously, we could find examples of all four possibilities: old import-competing industry regions; new import-competing industry regions; old exporting industry regions; and new exporting industry regions.

An excellent example of this phenomenon is provided by the case of steel. As can be seen in Table 1, steel production in Pennsylvania peaked in 1973. In 1963 Pennsylvania produced 23% of the U.S. raw steel; by 1983 it was producing only 15%. By contrast, even though steel production in the U.S. peaked nationally in 1974 at 145.7 million tons and has been declining ever since, steel production has expanded in both the central and southern states. In the 1981–82 downturn, the decline in production in Pennsylvania was more drastic than in the newer regions; during the recovery of 1983, production

Table 1. Raw Steel Production in One Old and Two New Regions, 1963–1983 (in millions of tons)

Year	Pennsylvania	Central States[a]	Southeast[b]	Total U.S.
1963	25.192	3.336	3.677	109.260
1964	30.515	4.076	3.826	
1965	31.998	4.425	4.010	
1966	32.122	4.528	4.196	
1967	29.881	4.644	4.268	
1968	31.018	4.810	4.453	
1969	32.791	5.300	4.674	
1970	30.031	5.198	4.629	
1971	27.655	4.908	4.601	
1972	30.416	4.843	5.562	
1973	33.925	5.596	5.743	
1974	33.535	5.751	5.619	145.720
1975	25.761	5.399	4.795	
1976	26.696	5.079	5.403	
1977	25.737	6.753	5.484	
1978	28.070	7.845	6.444	
1979	28.213	8.260	6.636	
1980	23.502	8.453	6.890	
1981	24.066	9.068	6.497	
1982	10.892	5.417	4.419	
1983	13.000	10.678	5.277	84.615

Source: Data is from the American Iron and Steel Institute, Washington, D.C.

[a] "Central states" denotes Minnesota, Missouri, Oklahoma, Texas, Nebraska, and Iowa.

[b] "Southeast" denotes Virginia, West Virginia, Georgia, Florida, North Carolina, South Carolina, and Louisiana.

recovered promptly in the newer regions, but was only anemic in Pennsylvania.

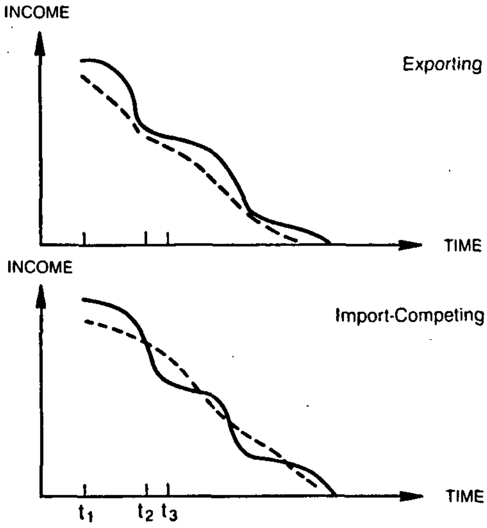
In the old regions regional income is falling, and so the return to place-specific assets declines as well, since the demand for these assets depends upon regional income. Less place-specific factors of production—new labor force entrants, new investment capital—emigrate from the old region. New regions will grow. The question arises, When will the interests of residents in both old and new regions coincide on tariff policy? The answer depends upon the level of economic activity.

The Business Cycle

We characterize the *business cycle* as exogenously induced fluctuations in the general price level. To the extent that labor's nominal wage is sticky in a downward direction, unemployment will result during cyclical downturns. Capacity utilization of plant and equipment will typically decline during the downturns. Since the business troughs are economy wide, both old and new regions may experience unemployment, although the old region will be more severely affected, since the plants in old regions typically have higher operating costs. Regional income thus moves with the general business cycle in both old and new regions. The cyclical income variations, however, trend around secular decline in the old regions and secular rise in the new regions, as illustrated in Figure 2.

The pattern of demands for protection that emerges depends upon how regional interests change over the business cycle, and how the changing interests translate

Figure 2. Real Regional Income in Old Regions, With and Without Tariffs (the broken line denotes "tariff")



into changing political behavior. Consideration of comparative advantage characteristics and the pattern of capital investment yields a 2 × 2 typology of regions: in comparative advantage terms, regions are import-competing or exporting; in capital investment terms, regions are either old or new.

Asymmetrical Effects of Protection over the Business Cycle

It is important to distinguish between the effects on a region of a change in the average level of protection afforded an import-competing industry over the course of a business cycle, and the changes in protection at different points in the cycle. A change in the average level of protection affects the equilibrium level of capacity in the industry. New regions, in which the industry's capacity is being located, have a vital interest in any change in the average level of protection afforded an industry. Old regions, which are losing capacity in any event, do not share this interest. They cannot expect a tariff increase to prevent the shift of capacity to other locations where profitability is higher.[6] *A fortiori*, while it is in the long-run interest of new regions to resist any declines in the average level of protection afforded their import-competing industry, the old region has no long-run interest in tariff protection, since, in the long run, the region's income will become independent of the national productive capacity of the given import-competing industry.

A variable of much greater political significance is the short-run economic consequences of a change in the level of tariff protection—even if actors expected an offsetting change in tariffs at a later time. The short-run effect of a change in the level of protection is to change the demand for output from existing capacity. Old regions, which have capital vintages with the highest operating costs, experience the greatest reductions in utilization rates when demand declines. Therefore, old regions have the greatest interest in increased protection at the trough of a business cycle, while at the peak of a cycle, when utilization rates are already high, an old region secures no additional regional income via an increase in tariffs. The asymmetry of the effects of protection on old import-competing regions is illustrated in Figure 2.

New regions will gain, but for an equivalent tariff these gains will not be any greater at the peaks than during the cyclical downturns. Furthermore, before a tariff is enacted and investment begins, new regions may not be aware that they will benefit from protection. After all, before the tariff there may be little or no capital—and, correspondingly, employment—in place in the region. Since the prospective gains are less visible, the new regions may be relatively less protectionist during the peaks than they would be if they had complete information about the effects of a change in tariff policy on investment in their regions. Whether they are is not central to our theory, but this possible uncertainty about the location of future activity does add an Olson-like twist to the argument. Tariffs are sometimes explained as occurring because the specific gains are obvious, but the general costs are uncertain and invisible. Here it is the gain to new regions from tariff protection that is uncertain, and so relatively invisible.

The distance between the "no tariff" and the "cum tariff" schedules in Figure 2 reflects the asymmetrical effects of protection in old regions over the business cycle. At the cyclical peaks, capacity utilization is high and the regional benefits of increased investment and production accrue only to new regions. There are nonetheless consumption costs associated with the higher tariff-protected prices, and so real income is lowered on this account. In Figure 2, at time t_1, for example, the tariff is accordingly shown on balance to lower real income at the peaks. During the cyclical troughs the consumption costs still lower real income. However, now there are offsetting gains in the old region, since the tariff leads to increased capacity utilization and regional employment, which in turn translates into increased regional income and demand for region-specific assets. Thus, at time t_3, for example, the tariff is shown to raise real income.

The discounted present value of the change in real income due to protection is represented by the difference between the cum tariff and no tariff schedules at each point in time, summed and discounted back to the present. For example, the addition to regional income from a tariff levied at time t_3 is given by the area between the two schedules beyond t_3. Of course a positive discount factor (interest rate) insures that the real wealth gain is not infinite, because income gains very far into the future receive very little weight.

Two forces now work to enhance the attraction of tariffs in cyclical downturns. First, during cyclical peaks, tariffs actually impose net income losses. The net gains during the downturn are pushed into the future and so, given a positive discount factor, the present value of future protection is diminished. Thus, for example, at t_1 a tariff levied immediately is of less present value to the old region than a tariff levied immediately at t_2 (McKeown, 1984).

Second, the distance between the no tariff and cum tariff schedules depends upon how much protection is extended to other regions in the overall tariff package. Each tariff levied on any commodity leads to higher consumption costs. Only if the commodity is produced locally and if there is local unemployment will there be compensating favorable consequences for regional industrial activity, and so gains to owners of place-specific assets. (In terms of Figure 2, each tariff for an industry that is not locally important lowers the cum tariff schedule. Indeed, high levels of broad protection can lower regional income below the no tariff income level.) This introduces a bias toward protection during business cycle troughs, since there is more to gain from protection for any particular old import-competing region. Consequently, the gains can remain positive for at least some protectionist coalitions, even though the consumption costs rise with each additional protected industry.

Industries thus find themselves in an environment wherein a tariff on their output holds widest regional appeal during general business downturns. During business cycle troughs there is a community of interest between old and new regions in securing tariffs to protect regional income. Conversely, during cyclical peaks old regions have no great interest in protection, since the gains from changing policy will accrue only to the new regions. Of course, tariffs carry costs for all regions, and the complicated problem for legislators is to secure protection for their own region's industry while giving up the least amount of protection possible for others, subject to the constraint of winning any vote.

Tariffs and the Business Cycle in Exporting Regions

Import restrictions on one set of goods reduce the relative prices of other goods. During business cycle peaks such restrictions, in conjunction with downward wage rigidity, will lead to at least temporary unemployment in some unprotected sectors and a decline in these regions' asset values. While import restrictions can theoretically increase aggregate domestic employment during the trough of a business cycle, we discount such a possibility, since "beggar-thy-neighbor" tariffs invite retaliation. Such retaliation suppresses foreign demand for exportables and insures that gains to import-competing regions will be offset by losses to export-oriented regions. Exporting (and other nonprotected) industries therefore lose rents from tariff-induced shifts in demand at every point in the business cycle.

Just as the imposition of tariffs has asymmetrical effects across old and new import-competing regions, the removal of tariffs has asymmetrical effects across old and new exporting regions. Old exporting regions, by definition, will not receive increased income flows during business cycle peaks from the removal of domestic tariffs on imports. At the business cycle peak it is new export regions that expand if tariffs are removed. Exporters will gain even more if domestic tariff reduction prompts a symmetric foreign response. Conversely, at the trough of a business cycle, an old export region may gain more than a new export region from tariff reductions.

Given the symmetry between old import-competing and old exporting regions, our theory implies that if the commonly alleged type of tariff cycle is observed, then the political weight of the old import-competing regions must exceed that of the old exporting regions. When import-competing industries are dominated by old regions, while exporting industries are dominated by new regions, the dynamics of tariff politics will produce a tariff cycle counter to the business cycle.[7]

Translating Economic Stakes into Political Demands

In this section we explore the political dynamics that arise from actors' stakes in achieving or eliminating protection. Although an industry may have a large economic stake in tariffs, its political behavior is mediated by its perception of the likelihood of political success. This in turn is a function not only of its own politically relevant resources, but also those of other actors, and the perceived likelihood that a winning coalition can be formed. Although an industry may obtain nontariff protection through unilateral appeals to the appropriate administrative agency, some nontariff and most tariff protection can only be obtained by some form of collective political action involving multiple sectors of the economy. In fact, governments committed to free trade might tolerate or even promote nontariff barriers awarded via administrative action, in order to divert the energies of actors with the largest stakes in protection so that they do not attempt to assemble a protectionist coalition. The central concern of this section is to identify any factors that impart asymmetries in the political strength of the four types of regions, and to sketch the possible coalition dynamics.

Political Resources and Collective Action Possibilities

One critical determinant of the level of political effort to be expected from a firm is whether it can reasonably expect its efforts to have a substantial effect on political outcomes. Such an expectation is reasonable if the firm is very large, for then even a modest proportion of its resources might be quite large, both in absolute terms and in terms of the total resources devoted to influencing political outcomes. Such an expectation would also be appropriate if the firm were acting in concert with other like-minded firms, so that their resources could be pooled to achieve a common goal. The latter condition can occur when firms have established a formal trade association, or have established a history of working together on political issues. In such circumstances, the well-known "free rider" problem could be at least partially surmounted.

Older regions are apt to be more politically potent than new regions, at least during the initial phases of an economic downturn. If, as Mancur Olson (1982) argues, the likelihood of being organized is an increasing function of the length of time firms in the same region have been established, then there is a prima facie basis for believing firms in old regions will be more politically organized than firms in new regions. If the size of firms is an increasing function of the length of time that they have been operating, then there are likely more very large firms in the old regions than in the new ones. This also biases political influence towards the old regions. Finally, as noted above, the future winners from tariff changes are much more difficult to identify than the present losers. As a consequence, organized interest groups representing the interests of new regions are much less likely to be politically significant than those interest groups representing the interests of old regions.

An additional asymmetry is temporal. In noting that the membership and general health of farm-oriented interest groups has tended to move countercyclically, Salisbury (1969) contends that members' contributions to such organizations constitute "luxuries" that they abandon in hard times. Coneybeare (letter to author, April 1983) notes that Chamberlin's (1974) work on collective goods provides a formal illustration of this argument. As depressions reduce incomes, under plausible income elasticities of demand, members will contribute less to organizations providing collective action benefits.[8] At first glance, this would apparently serve to undercut the political potency of all groups in cyclical downturns. However, if starting up a group is more costly and difficult than maintaining it, then the cyclical shifts in incentives for group contributions would tend to be more debilitating to those interests that had not yet formed groups than to those groups that already had (perhaps in a previous cyclical boom). The latter could conceivably survive the downturn, though with reduced contributions; for the former, the downturn might create an effective barrier to political entry. This is another factor granting an edge to old regions. Moreover, for organizations in old regions, the increased benefits accruing from collective action in times of depression will counteract, to some extent, the effect of declining incomes on members' contribution levels. As argued above, there is no obvious counterbalance available to the new regions.

The general effect of these asymmetries is to strengthen the hand of old regions as opposed to new regions. With more large firms, and more time in which to form trade associations, the old regions have an important political advantage.

Coalition Formation

Coalition formation is relevant to the analysis of the tariff cycle for two reasons. First, a coalition is generally necessary to achieve passage of tariff legislation. Industries seeking protection during periods of general prosperity may be forced to rely on unilateral action through bargaining with state agencies that can grant nontariff protection, because there are not enough other industries available to act as coalition partners. During a time when business conditions are generally depressed, there are many more potential coalition partners for such a protectionist bid than there are during more prosperous times. Thus, we posit an increase in efforts to enact protectionist legislation at such times simply because of the presence of more protectionist interest groups.

A second point regarding coalitions is that, because of multiplicity of winning coalitions may be possible, each with its own characteristic set of legislative side-payments, there could be a multiplicity of possible political outcomes for any given economic situation. In particular, it is important to understand how sectors that may not initially have a stake in tariffs may acquire tariff protection as a product of the coalition formation process.

A plausible initial presumption is that, because each import-competitive region desires a tariff only on its own output, it will be willing to support tariffs on other commodities only insofar as is necessary for forming and maintaining a winning coalition. Though such regions benefit from tariffs on their own commodities, they are of course hurt by tariffs on other goods. Conceivably, owners of regional assets will be worse off under "tariffs for all" than "tariffs for none"; thus, they will have a vested interest in holding the size of protectionist coalitions to the minimum necessary to win. Nonetheless, policies of "tariffs for all" are not uncommon: prominent historical examples are furnished by the Hawley-Smoot tariff of 1930, the German tariffs of 1879 and 1897, and the French tariffs of the 1830s and 1840s.

Important to understanding "tariffs for all" is Shepsle and Weingast's (1981) demonstration that uncertainty about being included in a winning coalition induces legislators to collude on "universalistic" allocation criteria for "pork barrel" legislation. When the probability of being excluded is sufficiently great, and the incidence of costs and benefits in one's own district meets certain minimal requirements, legislators will agree to avoid attempting to form minimal winning coalitions, and instead admit all who desire to join.

Another way universalistic coalitions may arise is in situations where legislators are not necessarily uncertain which coalition will win, but where there is no method whereby members of a minimal winning coalition can exclude other players from joining that coalition. Assume there is some possible coalition of import-competitive firms and regional actors that proposes a package of tariffs benefiting each region in that coalition. Assume further that the benefits to each member of the coalition are positive, and that the benefits to nonmembers of the coalition are negative. Under these circumstances, the temptation of nonmembers to join the potential winning coalition will be quite strong. A nonmember who joins the coalition changes his payoff—if the legislation passes—from some negative value to some less negative or positive value. Since the cost of being a nonmember increases as more and more commodities acquire protection, the pressures to join the bandwagon become stronger as more and more others do so. Meanwhile, even though riding on the bandwagon becomes less and less attractive as it becomes more crowded, no single rider finds it rational to dismount, since doing so will not change the voting outcome, and will only result in a worse negative outcome than would be obtained by staying on board (see, for example, Schelling, 1973).

An additional possibility is created

when some actors in the political system may have been excluded from full participation by the collusion of present participants. Limitations on the suffrage in the nineteenth century are a clear example of this; a similar phenomenon, though with less immediate applicability to tariff politics, is provided by the exclusion of Communist parties from governing coalitions. The current winners of protection may calculate that it is to their long-term advantage not to exclude any participants from a protectionist coalition, because to do so might aggrieve such participants so much that they would be tempted to alter the outcome by widening the circle of participation. Actors will therefore accept outcomes that are suboptimal in the short run, because of their fear of the long-run costs inherent in contending with the entry of new groups into the political process. If there are no groups outside of the process, this collusive dynamic obviously is absent. Such considerations seem to have played a role in French tariff setting in the period 1830-1860, and in the formation of the tariff coalition of iron and rye in imperial Germany.

When the above conditions do not obtain, and when victory is sufficiently probable that existing coalition members are no longer willing to dilute the benefits of protection, the coalition leaders will act to exclude some claimants for protection. The likely candidates for exclusion would be those commodities for which protection generates widespread and visible consumption costs and few regional benefits, and that bring relatively little political clout to a protectionist coalition (too disorganized, too concentrated in a few regions, not particularly important in any region).[9] Of course, import-competing actors excluded from a protectionist coalition can then improve their position in two ways: either they can join with the exporters to attack others' protection, or they can join the protectionist coalition at a later time.

The opposite situation in tariff coalitions is one in which there are very few potential members of a protectionist coalition. For example, in an economy characterized by overall growth, but with a small number of troubled industries, those industries in trouble might well face difficulties in assembling a large enough coalition to have any chance at all of obtaining any legislative action. Under these circumstances, we argue, such industries would be more likely to attempt to obtain some form of nontariff relief through administrative action, such as a petition for an International Trade Commission ruling to restrict imports in that industry.

An additional consideration in assessing likely coalition dynamics is the presence or absence of additional constraints or costs on the tariff issue arising from linkage to other foreign policy issues. Lowi (1964) argues that U.S. tariff politics after World War II changed in nature, because the greater U.S. role in world affairs led to much greater concern over interdependencies between U.S. tariff policy choices and outcomes on foreign policy issues not involving tariffs. For example, high levels of protection against European producers would have reduced their ability to finance rebuilding of their economies, and would have done nothing to correct the postwar dollar shortage. Such outcomes would have been inconsistent with the general foreign policy objective of establishing strong, capitalist economies in western Europe in order to resist both domestic communism and Soviet invasion.

It should now be clear that a wide variety of coalitional arrangements are consistent with the regional analysis which forms the core of our argument. Since so many forces come to bear on the actual enactment of tariffs, almost anything can happen in any particular case. However, our theory does imply a certain regularity in the pattern of protection, holding other forces constant. Empirical tests of the theory are therefore possible, at least in principle.

Implications of the Theory

Our basic hypothesis is that old import-competitive regions share an interest with new import-competitive regions in tariff protection for an industry only during periods of depressed demand. During such periods, tariff protection will induce a higher rate of employment and higher regional income for an old region than that region would otherwise experience. However, since old regions do not share in the growth of an industry during periods of prosperity, they have no interest in obtaining protection in prosperous times. Furthermore, since legislative politics are often based upon a mutual accommodation of regional interests, it follows that the probability of putting together a winning coalition of protectionist interests is greater near the trough of a general business cycle, when a would-be protectionist can find the maximum number of potential coalition partners. Conversely, during cyclical peaks, the old regions would not only have no incentive to seek new tariffs, but would even lack incentive to oppose tariff reductions, provided that a resulting increase in imports did not create idle capacity in the old region.

The strength of a given old region's interest in protection for a particular industry, and therefore the probability of a particular industry securing protectionist support, depends *upon the importance of* the industry output to the region's income. Therefore, industries in the early stages of a regional shift that still have a large fraction of their productive capacity in the old regions are more likely to secure protection during a protectionist wave than industries whose productive capacity is preponderantly in new regions. We have also argued that regional interests are more closely tied to the labor income than to the capital income generated by a given industry's output. It follows that industries generating large payrolls in such regions are more likely to secure regional support for protectionist demands than industries generating small payrolls in such regions. Moreover, since factor intensities are related to factor shares, other things being equal, more labor-intensive industries in old regions should be more successful in obtaining protection than less labor-intensive industries.

When old plants embody an older technology than that of new plants, variable costs of production are likely to be higher—or at least no lower—in the old than in the new regions. Consequently, a decline in demand for an industry's output will be met by a contraction in output in the old regions relative to the new regions. Therefore, if we look at several industries occupying both old and new regions, we will find that the greater the production cost differentials between regions, the greater the relative contraction and the greater the industry interest in tariff protection during a business downturn.

Another implication of a regional perspective on tariff policy is that when a country is sufficiently small in land area and labor is sufficiently mobile, the variability in tariff policies we have outlined will not occur. If the small country possesses a single industry, then that industry presumably will have already acquired what it regards as an optimum tariff; if the small country possesses multiple industries, the holders of place-specific assets will no longer have their fortunes tied to a particular industry.[10] For retailers and the like who possess region-specific assets but who are not directly employed in the affected industries, there similarly is no reason to bolster declining industries at the expense of rising ones. Essentially, the tariff cycle will disappear, because the spatial asymmetries that drive changes in protectionist sentiment over the business cycle will disappear.

Our theory argues that the effects of tariffs are spatially differentiated. The political implications of this for the organization of interests are twofold. First, we will find interest groups involved in tariff politics that are organized along geographic lines. Organizations such as state and local chambers of commerce, or regional government or business organizations in the United States, may have a rational basis for involvement in tariff politics, because of the regional impact of tariff policies. Second, interest groups organized along sectoral lines will tend to have cleavages along geographic lines. We may witness different regions possessing separate trade associations in the same sector—as occurred in the textile industry in the U.S. (Galambos, 1966)—or semiautonomous regional groupings within national sectoral trade associations—such as occurred in the German iron and steel industry in the nineteenth century (Boehme, 1966). Furthermore, the theory implies that the geographic regions represented in protectionist coalitions will change over the business cycle. Interest groups from old import-competing regions will allocate substantial effort to securing a tariff only during cyclical downswings, because only at that time will protection yield substantial benefits.

Finally, the theory does offer an alternative explanation to attempts to account for changes in the openness of the international trading system in terms of a theory of "hegemonic stability" (Gilpin, 1975; Krasner, 1976). Very simply, if business cycles in the nations of the world capitalist economy are synchronized, and if old import-competing regions generally outweigh politically the old exporting regions, then an increase in demands for protection during global downturns will occur throughout the states of the system. This makes it more likely that any given state will defect from a liberal order. It also makes it more likely that other states will respond to that defection by defecting also, since the costs of taking the "sucker's payoff" are now augmented by domestic pressures for a departure from an open system.

Relation to Previous Empirical Findings

Although we have not yet subjected our theory to empirical testing, we can gain some idea of its accuracy by examining whether its claims are consistent with previously published findings on the politics of tariffs. In this section we discuss several such studies, noting when the results obtained seem to be relevant to our theory and, if relevant, whether they are consistent or inconsistent with our argument.

Central to our argument has been the claim that when industries are enjoying healthy growth in demand, the pressures for protection from import-competitive interests will be reduced, since old regions—which, as we have argued, are especially likely to be politically potent—derive relatively little benefit from protection at such times. The finding of Cheh (1974) that individual U.S. tariffs are negatively related to growth rates for the sectors covered by the tariff is consistent with this argument. Riedel's (1977) finding that West German industries with high growth rates experienced relatively larger reductions in effective protection (but not in nominal protection) in the Kennedy Round is not inconsistent with our argument, but the case would be stronger if the relation obtained for nominal rates as well. Coneybeare's (1983) observation that the average 1971 tariff levels for a sample of 35 nations were negatively related to the rate of growth of gross domestic product (GDP) is also consistent with our argument. His finding that 1902 tariffs were positively related to the growth rate of GDP is vitiated by the fact that the values he used for growth rates were averages for the 42-year period from 1871 to 1913. A more reasonable procedure would have been to use the years just prior to the tariff observations. Takacs (1981) has analyzed requests by U.S. industries for nontariff protection, and finds that such requests are negatively associated with industry growth rates. Lavergne (1983) found that growth rates in U.S. employment had a moderately strong and expected negative relationship to protection only in the case of nominal U.S. tariff levels in 1964 and 1972, and for an index of nontariff barriers developed in the early 1970s. He observed a statistically significant but unexpected negative relation between growth rate in employment and the size of nominal tariff cuts made in the Tokyo Round, and obtained very weak and nonsignificant results for 1979 nominal tariff levels, effective tariffs in 1965 and 1972, and the size of Kennedy Round cuts. Marvel and Ray (1983) found that sectoral growth rates were positively related to reductions in sectoral U.S. tariffs in the Kennedy Round. They explain this in terms of a Peltzman-Stigler-Becker argument to the effect that rapid growth is a "windfall of good fortune," and that government officials will find it profitable to alter tariffs to share the windfall benefits with the consuming public. Apparently, their implicit argument is as follows: If a shift to high growth is associated with an increase in profitability, and if politicians obtain declining marginal political utility from actions that raise sectoral profits and increasing marginal political disutility from actions that increase consumers' losses, then the maximizing course for the government officials is to reduce the tariff for that sector. Our model does not investigate this supply side argument, but it does offer a complementary "demand side" explanation for the relation between growth rates and tariff protection.

A number of analysts (Constantopolous, 1974, and references therein; Helleiner, 1977; Pincus, 1975, 1977; Ray, 1981; and Reidel, 1977) have analyzed the cross-sectional distribution of protection in North America and western Europe. If we adopt the assumption that the amount of protection an industry receives is a positive function of its level of effective political demand, and that effective political demand is in turn a function of the size of regional effects, then the evidence is generally not inconsistent with our argument. Those receiving the most protection tend to be labor-intensive, low-wage industries. Labor intensiveness is consistent with substantial regional impact due to a large number of employees; low wage levels suggest sectors in western Europe and North America that are at a comparative disadvantage vis-a-vis competitors in the less-developed countries. However, low wages also imply modest effects on such regional variables as retail sales.

Pincus's (1975, 1977) analysis of the distribution of tariffs in the U.S. in 1824 finds that high tariffs are positively related to low proprietal income shares. He accounts for this in two ways. First, he argues that when the proprietal income share is low, the benefits to proprietors from a given tariff increase are larger than when proprietal income shares are high.[11] Second, he also seems to treat low proprietal income shares as a proxy for small numbers of proprietors (i.e., large firms), and then relies on an Olsonian argument about the greater ease of overcoming free riding in small groups. Our theory suggests a third reason why low proprietal income shares might be associated with effective demands for protection: low proprietal shares imply that, for a given size investment, the regional benefits are substantially greater than those that would be enjoyed by an industry with a high proprietal income share, because of the larger payments to local labor.

Another important implication of our

theory is that interest groups active on the tariff issue will be organized on spatial as well as sectoral lines. Most political science analyses of tariff politics have implicitly adopted the sector as the meaningful unit of analysis; attention to the spatial characteristics of interest group activity has been unsystematic and incidental to the primary focus on sectoral alignments. The problem of distinguishing spatial from sectoral activity is further complicated because sectoral associations can themselves be organized along spatial lines; if the analyst is not sensitive to the spatial characteristics of interest group representation, and does not investigate the possibility that certain geographic sections dominate a given interest group, it is not possible to determine from reading the analysis whether there has been a significant spatial component in interest group activity. Nonetheless, we can pick up a few clues by a close reading of the literature.

Pincus (1975) notes that demands for protection in 1824 were spatially concentrated—that is, petitions for a given tariff tended to originate in identifiable regions. He argues that the cost of interstate transport and communication necessitated such a pattern. In addition, a legislature constituted by representation of geographically compact districts would, in any event, tend to channel political activity along regional lines. Of course, such a pattern of spatial activity is also consistent with our weak general hypothesis that activity on tariff politics has a significant spatial component. Schattschneider (1935, p. 125) devoted considerable attention to the spatial characteristics of interest group representation. His conclusion—that when a sector is strongly sectionally concentrated, "wide secondary activity" on tariffs occurs—seems to support our contention that the regional effects of tariffs are politically significant.[12]

Summary

Our account of the tariff cycle is based upon the conventional premise that tariff policy is shaped by the rent-seeking behavior of interested parties.

At a national level, individuals whose assets are tied to import-competing industries have a direct interest in tariff protection for specific industries, regardless of the general level of economic activity. However, the gains to import competitors from tariff protection and the losses to exporters from retaliation and increases in input costs are greater when times are good than when demand is low. Given this symmetry of interests between import-competing and export-oriented industries at the national level, the countercyclic nature of tariff and other trade barriers appears to reflect interest group forces that are not explained by simple reference to nationwide industry interests.

We argue that an explanation for countercyclical tariffs lies in the fact that not all regions whose interests are tied to an import-competing industry share in the benefits of protection in the same way over a business cycle. In particular, old regions, which have lost their locational advantage, cannot expect to share in the rents generated by protection when the general level of activity is high. However, when the industry is generally depressed, it is the older regions, where a large fraction of the idle import-competing capacity is located, that will benefit disproportionately from tariff protection. Therefore, an old region is more likely to join a protectionist coalition when its industrial base faces poor economic conditions than otherwise. Further, when an old region supports a protectionist coalition it is likely to be willing to endure higher costs (through the broadening of the set of industries for which protection is sought) than is a new region, since when it supports protectionism its own expected gains from the reduction of idle capacity are greater than those of a new region.

The political dynamics of tariff seeking tend to reinforce the central role old regions play in the process. When an industry is a major part of the economic base of a region, citizens of that region who have a stake in the general economic conditions in that region are the industry's natural allies. The industry need not spend much of its own resources to secure political support for its demands from regional political representatives. By contrast, in areas where an industry might expand if given tariff protection, that industry need not be a major employer currently. Further, no region can be certain that a given industry will expand within its borders. Consequently, the industry has fewer natural allies outside its old regions, and must expend its own resources in those regions if it is to secure political support of their representatives.

To summarize, differences in regional interests create the following general dynamics: When general economic conditions are poor, the old import-competing regions will join the protectionist coalition, and pressures for tariffs will increase. Although export-oriented old regions will be joining a free-trade coalition at the same time, in a political system where old import-competing regions outweight old exporting regions, the activation of the old regions on both sides will favor the protectionists. Conversely, during cyclical peaks, when both types of old regions desert their respective coalitions, the protectionist coalition will suffer a relatively greater loss in political strength. It is thus at the peaks that the free traders have their best winning chances.

Our argument also has a more general implication for the study of interest group activity. If our model is correct, the level of interest group activity on tariffs and many other forms of rent seeking ought to fluctuate with business conditions, with intense activity most likely to occur at a cyclical trough. Such a relation between business conditions and interest group activity may account for the often-noted disparity in the findings of Schattschneider (1935) and Bauer, Pool, and Dexter (1963) on the levels of activity of interest groups. Whereas Schattschneider, studying decision making at the onset of the Great Depression, paints a picture of powerful, aggressive interest groups, the Bauer, Pool, and Dexter study, performed in the period from 1954 to 1962, portrays interest groups as having limited resources, and as paying limited attention to the tariff issue. The greater prosperity of the latter period may well account for the difference in reported behavior.

Notes

An earlier version of this paper was presented at the 1984 annual meetings of the American Political Science Association and the Public Choice Society. We have received helpful comments from John Conybeare, Joanne Gowa, Miles Kahler, Robert Keohane, Howard Marvel, Jonathan Pincus, Susan Strange, and Robert Walters.

1. This argument neglects the possibility that under some circumstances—in particular those addressed by arguments about "optimal" tariffs—protection might actually raise national income.

2. This is not the only asymmetry that could do so, but it is one asymmetry whose presence seems endemic to private enterprise market systems. Other asymmetries—for example, conflicting interests among owners of capital due to different owners holding portfolios concentrated in different firms—can in principle be eliminated by the dominance of large institutional investors that hold highly diversified portfolios.

3. The postulate of elected representatives from geographically defined districts is adopted for its simplicity and familiarity to our readers. However, there is no logical necessity that the phenomena we discuss occur only in such systems. The minimal institutional conditions that seem necessary in order for the process we describe to occur are as follows: First, property rights must be generally private. Second, the government must not be a perfect dictatorship, lacking any motivation to respond to the political demands of property owners. Such minimal conditions can be met without any form of mass suffrage or a formal legislature. The point is of considerable empirical significance, since extensive

private property rights often occur in systems lacking mass suffrage or significant electoral competition.

4. Owners whose assets were concentrated in capital of differing vintages—and hence different production costs—could have different political interests in achieving protection analogous to the distinction developed here between interests in old and new regions. We have not constructed a model based on the lack of diversity in capitalists' portfolios, because the current predominance of institutional investors with highly diversified portfolios suggests this is an unlikely assumption for a theory purporting to explain something about current tariff politics. However, for earlier eras, when capital markets were less well developed, an approach to the problem that takes concentrated portfolios as the rule rather than the exception appears more reasonable.

5. Generally, the costs incurred in changing jobs are greatly outweighed by the decrease in earnings arising from the difficulty in fully employing accumulated human capital in a different job (Stern, 1972). Because of this, labor can suffer significant decreases in earnings, even when discharged during periods of tight labor markets, when the costs of finding a new job are not large.

6. There is one important exception to this generalization: if there were no new import-competing industry regions—that is, if there were no significant regional cost differences in a given industry, and the industry's capital stock had been running down—then an increase in tariffs could benefit the old industry regions. This is because any resultant expansion in industry size would have to occur in what had been an old region. Under this circumstance, old industry regions would have some incentive to seek protection, even when existing capacity was fully utilized.

7. A product cycle theory of international trade (Vernon, 1966) provides a basis for believing that industries dominated by old regions will be import-competing, while industries dominated by new regions will be exporting.

8. Normal goods, with income elasticities of demand exceeding unity.

9. Pincus (1975) notes in his empirical study of the U.S. tariff of 1824 that industries with low proprietary income shares did well in obtaining protection. Presumably, this is because firm purchases of labor power and input commodities accounted for a very large proportion of firm disbursements. Such a pattern of disbursements would generate greater regional benefits than those of a firm with an equivalent level of capitalization and rate of profit, but a smaller payroll or more modest needs for input commodities. Pincus also associates the complements of these last-mentioned characteristics with the attainment of protection.

10. An exception, of course, is the worker who possesses industry-specific human capital and who cannot readily migrate.

11. This follows simply from the observation that profits are credited to investments rather than to sales. A given percentage increase in revenue will increase the rate of profit of a small investment much more dramatically than that of a large investment.

12. Schattschneider also notes several reasons for firms within the same organization taking different stands on tariffs: internal transport costs, product differentiation, and interest groups whose organizing principle is some issue or concern other than tariffs.

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ON THE POSSIBILITY OF FAITHFULLY REPRESENTATIVE COMMITTEES

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By faithful representation *we mean the delegation of decision making to a relatively small committee that, using a weighted voting rule, will for each pair of alternatives make sincere choices identical to those that would be made by the society as a whole, and with the same vote margins. We show that for any society, no matter how large, faithful representation is possible by a committee with no more than* $m(m-1)/2$ *members, where* m *is the number of alternatives. We also show that for any society, no matter how nonideological the bulk of its electorate, social preferences can be faithfully represented by a committee whose members all have single-peaked or single-troughed preferences. Thus, all societies can be faithfully represented by a committee whose members see the world in unidimensional terms—that is, representatives can share a coherent ideological perspective even though the electorates they represent lack such a perspective. We further show that the usual mechanisms of proportional representation and the modified form of proportional representation recently proposed by Chamberlin and Courant (1983) do not guarantee faithful representation, and we discuss mechanisms that may provide faithful representation, even in a context in which new alternatives can arise.*

Faithful representation has long been advocated as the ideal form of representation. As Chamberlin and Courant (1983, p. 718) characterize this view, "Representative democracy is at best a working model of direct democracy and is most successful when it generates . . . decisions as close as possible to those that would be generated in a direct democracy."[1] We shall show that, contrary to what common sense might expect, even societies having millions of voters (and whose voters have millions of distinct preference orderings over the various alternatives) can often be *faithfully represented* by a relatively small group of representatives: i.e., the representatives' weighted sincere votes will perfectly mirror the directions of all pairwise preferences between alternatives, and the margins by which the voters prefer one alternative over another. Moreover, we shall show that the means of implementing such faithful representation is not any of the usual forms of proportional representation.

We believe that the idea of faithful representation is of theoretical and practical importance. Direct democracy is rare; usually voters must choose a committee (e.g., a legislature) to decide on their behalf (Black, 1958). The size of electoral margins may influence policies over and above the simple directionality of vote outcomes. Vote margins may affect future policy proposals as well as perceptions of the likelihood of reversal of present decisions.

There are many instances of the importance of vote margins. For example, New York State school budgets are voted on by residents of school districts, but the subsequently proposed budgets are determined by the school boards. Feld and Grossman (1984) show that the size of the budget referendum majorities affects the subsequently proposed budgets over and above the decision to either pass or defeat a particular proposed budget. Journalists, political scientists, and politicians themselves commonly use an elected official's margin of victory as an indication of the size of his or her political mandate.[2] Observers and participants in legislative decisions use legislative margins as an indication of the stability or changeability of a particular policy direction (Grofman and Uhlaner, 1985). A strong notion of representation requires that a committee not only make the same decision as would the entire set of voters (we shall refer to this as *order representation*), but also that it would make decisions by the same margins, so as to indicate the strength as well as the direction of preference of the voters. We shall refer to the latter form as *faithful representation.*[3]

It might appear that faithful representation is impossible. Certainly the social choice literature is filled with impossibility results, and faithful representation seems to be such a strong requirement that an impossibility result would seem almost inevitable. As we shall see, however, although the usual forms of representation (e.g., plurality or list proportional representation) cannot guarantee faithful representation, it is possible to devise a weighted voting scheme that will in general provide faithful representation. A *weighted voting scheme* is one in which each committee member has multiple votes to cast. Such schemes are used in proxy-voting for corporate boards of directors, in voting for labor union officers in labor federations such as the AFL-CIO, in most county governments in the State of New York, in the European Economic Community, and in various other organizations (Brams, 1975; Grofman and Scarrow, 1981, 1982; Straffin, 1980). Moreover, the committee size needed to implement a weighted voting scheme to carry out faithful representation is remarkably small; and the committee can be constructed so that all of its members will have single-peaked or single-troughed preferences with respect to any given linear ordering, even though in the society as a whole only a handful of voters have orderings that are single-peaked or single-troughed with respect to that range of alternatives.[4] Thus, we have the quite counterintuitive result that we can have faithful representation from a legislature whose members have well-defined ideologies underlying their preferences (i.e., they have single-peaked or single-troughed preferences with respect to a given linear ordering) even though the society they faithfully represent is not at all ideological in character.[5] Furthermore, a committee that is selected to be faithfully representative for a relatively small set of alternatives can, under reasonable circumstances, continue to be faithfully representative of the electorate over a much wider set of alternatives that may subsequently arise.

Proportional Representation is Not Faithful Representation

Proportional representation in multimember districts is generally considered an improvement over single-member district-based plurality (or majority runoff) representation, because the legislature or committee chosen is expected to mirror more closely the composition (and thus the preferences) of the electorate. However, list proportional representation (list PR), the most common form of PR, is generally inadequate for faithfully representing pairwise vote margins for more than two alternatives, especially when multiple alternatives cannot be meaningfully ordered along a single continuum.

We shall treat a political party as equivalent to a given preference ordering over alternatives (i.e., each party representative will be expected to vote in accord with the same linear ordering). List PR is inadequate to provide faithful representation, because there are generally too few parties to represent the variety of preference orderings in the electorate, and voters are forced to choose the closest approximation to their own ordering (e.g., relying only on first preferences); the process of approximation can severely distort the outcomes. For example, consider a situation with three alternatives—*A*, *B*, and *C*—and three parties—X, Y, and Z—each associated with the modal preference orderings as follows: X:*ABC*, Y:*BCA*, Z:*CAB*.

Now consider that the population has the full range of preference orderings in the proportions shown in the example below:

Voter Preference Orderings	*Votes*	*Most Likely Party Choice*
ABC	23	X
BCA	20	Y
CAB	20	Z
CBA	15	Z
BAC	17	Y
ACB	5	X

In this example we also have shown the party that most closely approximates the preferences of voters with each preference ordering. If parties are given votes in proportion to their choice by voters, then X, Y, and Z have 28, 37, and 35 votes, respectively. No party has a majority on its own; their votes on issues result in the collective choices of *A* over *B* by 26, *B* over *C* by 30, and *C* over *A* by 44. This is in contrast to very different preferences of the voters, *B* over *A* by 4, *B* over *C* by 20, and *C* over *A* by 10. One of three decisions is made in the wrong direction; all of the voter margins are incorrect, and the majority preference ordering of the voters is *BCA*, while that of their chosen representatives is a cycle.

This simple example shows the inadequacy of list proportional representation. With additional political parties the system might work better, but there is no guarantee that the most representative set of parties will emerge; it is probably realistic to expect that there will be a few parties representing the modal preferences, just as in our example. Even if other parties are included, the choices and vote margins might still be distorted as long as some voters are forced to choose a party that does not perfectly represent them. These findings seem to suggest that proper representation requires as many different representatives as there are preference orderings in the population. Indeed, at least one knowledgeable political scientist suggested to us that the search for faithfully representative committees was apt to be fruitless for exactly this reason: He conjectured that if the society was large, only a large committee (presumably one representing most of the preference orderings in the population) would be able to represent it faithfully. For *m* alternatives, there are *m*! different possible preference orderings; for 12 alternatives, everyone in the United States could have a different preference ordering. Even if people had only ideologically consistent orderings (i.e., single-peaked preferences over the continuum defined by a particular linear ordering), there would still be 2,048 different preference orderings. If any such large number of preference orderings had to be represented directly in a committee, a system of faithful representation would be unworkable. Fortunately, we can show that faithful representation can always be achieved with a relatively small number of representatives (e.g., a maximum of 66 in the case of 12 alternatives), no matter how many voters there are.

Basic Theorems on Faithful Representation

McGarvey's (1953) interest in demonstrating the possibilities of vote paradoxes led him to show that any possible set of pairwise preferences over m alternatives (and thus in particular any cycle) can be reproduced with at most $m(m-1)$ different preference orderings. Since the task of constructing vote paradoxes is of little more than technical interest, this result is almost unknown even in the social choice literature. However, it is an immediate corollary of McGarvey's theorem:

COROLLARY TO MCGARVEY'S THEOREM. *For any society whose members have preference orderings over m alternatives, there exists a set of at most $m(m-1)$ representatives whose majority choices between every pair of alternatives perfectly mimic the directionality of the pairwise majority choices of the society as a whole.*

We shall call a committee that mirrors the pairwise majority choices of a larger group an *order-representative* committee. McGarvey's result tells us that we can obtain an order-representative committee with at most $m(m-1)$ representatives. Order representation is often relatively simple to satisfy. For example, if a society has a transitive ordering over alternatives, then a single individual with that set of preferences is order representative for that society. As McGarvey shows, even if the group majority preferences are not transitive, then one can generate a pair of preference orderings that produces the majority for any alternative over any other without affecting the results of any other binary choices. This can be seen by considering five alternatives, *A* through *E*, and the decision between *A* and *B*. If someone has ordering *ABCDE* and someone else has ordering *EDCAB*, then together they prefer *A* to *B* and cancel out each other's preferences (they are effectively indifferent) over all other binary choices.

This analysis contains the essential logic needed to extend McGarvey's results from order representation to faithful representation. McGarvey's argument can be easily extended to show that faithful representation (of any margins) is possible by weighting each member of the pair of preference orderings used to produce a binary choice (*ABCDE* and *EDCAB* in the example above) by half the margin; then, together, they produce the particular margin while all other representatives cancel out one another on this binary choice. This extension of McGarvey's Theorem indicates that a maximum of $m(m-1)$ different preference orderings are sufficient to represent any group faithfully. We can, however, prove the stronger result that this same representation can always be achieved with half that many, or $m(m-1)/2$, different preference orderings.

THEOREM 1. *A maximum of $m(m-1)/2$ different (weighted) preference orderings (i.e., a set of $m(m-1)/2$ committee members voting sincerely, each in accord with a distinct preference ordering) is sufficient to represent the margins of pairwise preferences over m alternatives that emerge from any* *group—that is, to faithfully represent the group.*

Proof. Two different forms of proof provide insight into this theorem, and so are worth some discussion.

First, reconsider our example. Let us try to faithfully represent this situation with preference orderings ABC, BCA, and CAB. It is easily seen that the margins of preference of A over B are the result of adding up the frequencies of all individual orderings preferring A over B, and subtracting the frequencies of all individual orderings preferring B over A, as shown below:

For $A > B$: $f(ABC) - f(BCA) + f(CAB) = -4$
For $B > C$: $f(ABC) + f(BCA) - f(CAB) = +20$
For $A > C$: $f(ABC) - f(BCA) - f(CAB) = -10$

Rows two and three are similarly based upon the computation of the margins of B over C and A over C, respectively. We have three independent equations and three unknowns that can be solved for the frequencies of each of these preference orderings; all three are necessary to reproduce these particular margins of preference. In this case the solution is

$f(ABC) = 8$,
$f(BCA) = 23$,
and $f(CAB) = 11$.

This example shows how faithful representation can be produced, and also (as per our previous discussion) that proportional representational procedures can distort decisions, even though faithfully representative weights do exist. As noted previously, in this example, weights derived from a list form of proportional representation give rise to cyclical majorities, even though the electorate as a whole has a transitive majority ordering.

In general, there is one equation for each of the $m(m - 1)/2$ binary choice margins, and so the equation set can be solved—with exactly as many equations as unknowns—so long as the equations are independent. Because it would be undesirable to give a representative a negative weight, if the solutions of the equations turn out to be negative, a representative assigned a negative weight can be replaced with one having exactly the opposite preference ordering who is given a positive weight. (By the *opposite preference ordering* we mean one which reverses each pairwise preference: e.g., the opposite of ABC is CBA.) Because of the independence requirement, not every set of $m(m - 1)/2$ preference orderings can be used to faithfully represent a group. On the other hand, the set of preference orderings that can be used to obtain faithful representation is not, in general, unique.

The second form of proof is by construction, using McGarvey's logic to construct a set of preference orderings (and frequencies/weights for each) that reconstruct a given set of margins. Let P_{ij} indicate orderings in which i is to the left of j, which we also denote as $i < j$. There are $m(m - 1)/2$ different preference orderings of the form

$$\begin{aligned} P_{ij} = {} & [i+1, i+2, \ldots j], [i], \\ & [j+1, j+2, \ldots m], \\ & [i-1, i-2, \ldots 1]. \end{aligned} \tag{1}$$

This apparently complex formula is actually a specification of a simple idea that can be illustrated for four alternatives: That is, $P_{1,2} = 2\,1\,3\,4$; $P_{1,3} = 2\,3\,1\,4$; $P_{1,4} = 2\,3\,4\,1$; $P_{2,3} = 3\,2\,4\,1$; $P_{2,4} = 3\,4\,2\,1$; and $P_{3,4} = 4\,3\,2\,1$.[6]

Each ordering and the negative of the next constitute a McGarvey pair; e.g., *2 1 3 4* and *4 1 3 2* (the reverse of *2 3 1 4*) cancel each other out, except for their both contributing to the margin of *1* over *3*. Thus, McGarvey pairs for the $m(m - 1)/2$ margins can be constructed from these $m(m - 1)/2$ preference orderings by using each one twice. This proof by construction provides an example of a set of representatives that can faithfully represent any set of margins.

Just as in the previous proof, the frequencies of each of these preference orderings necessary to reproduce the margins can be negative. Consequently, as above, any preference ordering that requires a negative weight would be replaced with its opposite with a positive weight. This is theoretically important, as we shall show below after we provide an extension to Theorem 1.

Theorem 1 says that one can faithfully represent the margins with $m(m - 1)/2$ weighted preference orderings. This procedure does not faithfully reproduce a 25-vote margin of *A* over *B*, however, this might have been 25 out of 2 million in the electorate, and be 25 out of 100 in the committee of representatives. Fortunately, only one additional preference ordering is required to reproduce the proportions as well as the margins themselves.

COROLLARY TO THEOREM 1. *A maximum of $m(m - 1)/2 + 1$ different weighted preference orderings is sufficient to represent the proportionate margins of pairwise preferences that emerge from any group—that is, to represent faithfully the group margins and their proportions of the total votes.*

Proof. The first proof of Theorem 1 can be extended to prove this corollary. We merely include one additional preference ordering and add one more equation specifying that the frequencies of all of these preference orderings add up to the total number of votes in the electorate.[7]

Our second proof of Theorem 1, the proof by construction, has further implications. It should be noted that every preference ordering given in the proof above is single peaked with respect to the ordering from *1* to *m* (*1* to *4* in the example). The opposite preference orderings are all single troughed with respect to the same ordering. (The opposite of a single-peaked ordering is always a single-troughed ordering with respect to the same continuum.) Consequently, this second proof constitutes a proof for an even stronger theorem:

THEOREM 2. *With respect to any linear ordering, a maximum of $m(m - 1)/2$ different (weighted) single-peaked and/or single-troughed preference orderings are sufficient to represent the group decisions faithfully over m alternatives.*

The Preferences of Members of Faithfully Representative Committees and Those They Represent

Since there are various faithfully representative committees, it would be useful to know whether the preference orderings of the members of such committees are necessarily similar to those of the represented population. The answer is there need be no overlap at all—a set of representatives can faithfully represent a group and yet have no overlap with the set of preference orderings of the group it represents.

Consider a society with a single individual with the preference ordering *3 6 5 1 2 4*. The committee shown below faithfully represents that society, but has no overlap with it.

Ordering	*Weight*
6 5 4 3 1 2	1
2 3 1 4 5 6	1
6 5 1 4 3 2	1
3 2 4 5 6 1	1
1 6 5 2 4 3	1
3 4 5 6 2 1	1
1 2 3 6 5 4	1

However, if there are only three alternatives, no committee can faithfully represent a society of a single person unless it includes that person's preference ordering.

The proof by construction for Theorem 2 showed how one can construct a set of preference orderings that can faithfully represent any group. If we have enough orderings to choose from, we can simply choose our committee so as to exclude from its members any with preference orderings in the society being represented.

COROLLARY TO THEOREM 2. *If a society has no preference orderings which are either single peaked or single troughed with respect to some given ordering, then that society can be faithfully represented by a committee whose preference orderings do not overlap those in the society.*

Proof. Use the second construction method used in the proof of Theorem 1, with the single-peaked ordering based on the single-troughed preferences not found in the society. Q.E.D.

This corollary can also be regarded as a special case of the more general theorem given below.

THEOREM 3. *If a society lacks at least $m(m-1)/2$ linearly independent preference orderings (i.e., orderings that generate linearly independent equations) from among the orderings held by any of its members, then it can be faithfully represented by a committee whose preference orderings do not overlap those in the society.*

Proof. Use the equational method used in the first proof of Theorem 1. (It may be necessary to give some members negative weights.) Q.E.D.

On pragmatic grounds, one would prefer a faithfully representative committee whose members did share preference orderings with the underlying electorate to one that did not. Fortunately, it is always possible to have a faithfully representative committee composed largely or entirely of preference orderings in the represented electorate.

THEOREM 4. *There is always a subset of no more than $m(m-1)/2$ of the set of preference orderings in a group that can be used to faithfully represent the group as a whole.*

Proof. If the society has less than $m(m-1)/2$ members, we are done. If not, set up $m(m-1)/2$ equations as in the first method used to prove Theorem 1, but using the complete set of n preference orderings in the group. Clearly, this set of equations has a solution, namely, that in which each ordering is given the weight equal to the number of group members holding the ordering. Of course, we have too many variables in our equation set. However, a well-known result in linear algebra tells us that since these equations do have a solution, there will exist at least one subset of no more than $m(m-1)/2$ of these variables that will be sufficient to determine the $m(m-1)/2$ margins,[8] since the rank of the $m(m-1)/2$ by n matrix specified by this equation set cannot exceed $m(m-1)/2$. Q.E.D.

It might appear that the faithfully representative subset of a group should, as much as possible, be representative of the full range of preferences of the group, but this may be unrealistic, because almost all orderings are held by some members of the society. Also, there is some reason to want a set of representatives whose preferences are more "coherent" than the probably widely divergent preference orderings held by the public. In particular, it would be nice to find a committee all of whose preferences are single peaked over some particular unidimensional continuum, since knowledge of this fact might simplify decision making in the group. We now turn to consider the relationship of single-peakedness to faithful representation.

Single-peakedness, Net Margins, and Faithful Representation

Knowledge of the paradox of voting, combined with the recognition of its empirical rarity, has frequently led researchers to suppose that there is coherence in individual preference orderings. Specifically, it has often been noted that if there is an underlying ordering of alternatives such that all individuals have single-peaked preference orderings with respect to the unidimensional continuum defined by that ordering, then the group as a whole has transitive majority preferences. Unfortunately for political theorists, few groups have individual preferences that are all single peaked. Majority decisions, however, may be transitive even though there are many individual preferences that are not single peaked (Feld and Grofman, n.d.; Niemi, 1969). We believe that the criterion of single-peakedness is too restrictive to be applicable in most actual situations, but that its weaker counterpart at the aggregate level, which we call the *Net Margin Condition,* is much more commonly found.

DEFINITION. *The Net Margin Condition is said to apply to a group's preferences over a set of alternatives, if and only if there is some linear ordering of alternatives such that for every* i, j, *and* k *arrayed* $i < j < k$ *with respect to that ordering,* $m(i,j) \leqslant m(i,k) \leqslant m(j,k)$, *where* $m(i,j)$ *is the margin by which alternative* i *is preferred to alternative* j *(negative if alternative* j *is majority preferred to alternative* i*).*

The definition may be made more meaningful with the diagram shown below. The alternatives are listed according to the linear ordering across the top and down the side. The entries in the matrix are just the margins $m(i,j)$. Those below the main diagonal are just the negative of the corresponding one above—e.g., $m(1,2) = -m(2,1)$; only those above the main diagonal are shown. The Net Margin Condition requires that the entries increase (or at least do not decrease) as one moves to the right or down within this half of the matrix of margins.

Matrix of Margins

$$\begin{array}{cccccc} & 1 & 2 & 3 & 4 & .. \\ 1 & & m(1,2) & m(1,3) & m(1,4) & \ldots \\ 2 & & & m(2,3) & m(2,4) & \ldots \\ 3 & & & & m(3,4) & \ldots \\ \cdot & & & & & \\ \cdot & & & & & \end{array}$$

Single-peakedness at the individual level implies the Net Margin Condition at the aggregate level. This can be seen by the implication of single-peakedness that everyone who prefers *1* to *2* must also prefer *1* to *3*, so the number of people voting for *1* over *3* must be at least as great as the number voting for *1* over *2*.

It is easily seen that the Net Margin Condition can hold while only a single individual has preferences that are single peaked with respect to the linear continuum along which margins are to be ordered; consider that all but one member of a group have preferences that are not single peaked, half of them having preferences opposite those of the other half, thus cancelling one another out on all binary decisions. (If m is large enough, we can easily satisfy this condition.) The one other member determines all of the margins; if that one has single-peaked preferences (i.e., if he has a transitive ordering of his pairwise preferences), then the margins are defined by his single-peaked preferences and therefore must satisfy the Net Margin Condition.

Moreover, the Net Margin Condition does not require any of the individuals to have single-peaked preference with respect to the linear continuum along which margins are to be ordered. For example, the Net Margin Condition for the seven-member committee discussed above obtains for the ordering *3 6 5 1 2 4*, even though none of the members of the committee have single-peaked preferences with respect to that ordering.

The Net Margin Condition permits a special form of faithful representation as expressed in Theorem 5:

THEOREM 5. *A group can be faithfully represented by a committee composed entirely of individuals with single-peaked preferences, if and only if group preferences satisfy the Net Margin Condition. In particular, for groups satisfying the Net Margin Condition, a maximum of* $m(m - 1)/2 + 1$ *different weighted single-peaked preference orderings is sufficient to faithfully represent the group margins and the proportions of the total votes cast on each pairwise choice.*

Proof. We have already proven that single-peaked preferences for all individuals in the society imply the Net Margin Condition for the group. In this situation, single-peaked preferences of the faithfully representative committee imply that the Net Margin Condition applies to the committee margins. Since the committee faithfully represents the group, the committee margins are those of the group, so the group must obey the Net Margin Condition. The other direction is proven by construction, as shown in the Appendix. We show that we can construct a set of single-peaked preference orderings that can faithfully represent any group whose margins satisfy the Net Margin Condition.

The Net Margin Condition is sufficient but not necessary for there to be a transitive majority ordering.[9] Thus, a group may have a transitive majority preference ordering but not be faithfully representable exclusively with single-peaked preference orderings, because the group fails to satisfy the Net Margin Condition (cf. Feld and Grofman [1986]).

Theorem 5 is important in that it indicates that a group having few single-peaked preferences with respect to any ordering can, if the Net Margin Condition is met, be faithfully represented by a committee whose preferences are all single peaked with respect to some given ordering. We believe that elected representatives are much more likely to hold "ideologically coherent" positions (on, say, a left-right dimension), and thus to have single-peaked preferences, than are their constituents. Empirical studies show that American voters hold a wide variety of preference orderings, including many that are not single peaked. We suggest that the large differences between voters and their representatives in the diversity and degree of ideological consistency of each group's preference orderings provide no obstacles to faithful representation.

The January 1980 National Election Study conducted by the University of Michigan Center for Political Studies provides an empirical example of a situation where the aggregate margins of preference satisfy the Net Margin Condition, even though there were voters who had every possible preference ordering over a given set of candidates. In the next section, we show one way that this electorate could have been faithfully represented by a committee composed entirely of representatives having single-peaked preference orderings.

An Empirical Example

In 1980, the University of Michigan Center for Political Studies undertook a panel study of voters preceding and immediately following the 1980 presidential election. On the first wave of that study, in January 1980, individual respondents were asked to indicate their feelings towards each of the candidates by indicating a number from 0 to 100 (where 100 is the most positive feeling). These data allowed us to determine a preference

Table 1. Proportions Favoring the First Candidate of Each Ordered Pair

Candidates	Candidates: Kennedy	Carter	Ford	Reagan
Kennedy	—	36.1	36.5	40.5
Carter		—	48.4	53.7
Ford			—	54.6
Reagan				—

Note: Proportions below the main diagonal are omitted: values would be 100 minus the proportions in the corresponding cell above.

ordering over the candidates at that time. The survey included a long list of candidates; for the present purposes, we confine our attention to the four best known candidates: Carter, Kennedy, Ford, and Reagan. Most voters indicated that they recognized the names of all these candidates.

Based upon our own knowledge of this situation, we determined that if there was an ideological (left-right) continuum, it would have to be Kennedy, Carter, Ford, and Reagan, from left to right respectively. There are a total of 24 different strong preference orderings over four alternatives. All individuals who indicated ties or did not give an answer for one candidate were excluded to simplify the analyses and interpretations. As we show elsewhere (Feld and Grofman, 1985), every one of the 24 possible preference orderings was represented in the sample of voters, and almost half (47%) of the preference orderings were not consistent with single-peakedness on the given hypothesized continuum.

Despite the large proportion of preference orderings that were not single-peaked, the margins of pairwise preferences conformed perfectly to the partial ordering expected to arise from single-peaked preferences, thus satisfying the Net Margin Condition, as shown in Table 1.

According to Theorem 5, if the Net Margin Condition is satisfied, a group can be faithfully represented by $[m(m - 1)/2] + 1$ single-peaked preference orderings. In this case, the margins in Table 1 can be reproduced by the following frequencies of seven single-peaked preference orderings, as can be determined by following the algorithm described in the Appendix:

36.1% of *KCFR*
.4% of *CKFR*
4.0% of *CFKR*
7.9% of *CFRK*
5.3% of *FCRK*
.9% of *FRCK*
45.4% of *RFCK*

This provides an empirical example of an electorate not limited to single-peaked preferences that can nevertheless be faithfully represented by a committee whose members all have single-peaked preference orderings.

Procedures to Provide Faithful Representation

As Chamberlin and Courant (1983, p. 718) point out, although there is a sizeable comparative politics literature on the PR vs. plurality debate, "social change theorists have paid almost no attention to methods of selecting representative bodies." This article, like theirs, seeks to remedy that surprising omission. Since it is always possible to faithfully represent the preferences of voters, the question is how this might practically be done. Unfortunately, as we showed with respect to the list form of proportional representation, current electoral systems do not necessarily provide faithfully representative legislatures; they either fail to choose a correct set of legislators or fail to weight committee members properly to produce faithful representation of the population. The same problems apply to the single transferable vote method of PR (STV) (Sugden, 1984) and to the form of PR proposed by Chamberlin and Courant (1983), although Sugden (1984) shows that if all voters have single-peaked preferences, STV will be order representative. The Chamberlin-Courant method may pick a committee that is not even order representative. Consider a seven-member society with preferences among three alternatives, as shown below:

Number of Voters		
3	1	3
A	B	C
B	A	B
C	C	A

The Chamberlin-Courant rule for a committee of size 2 would pick a committee with four votes for A and three weighted votes for C. This makes A dictator. However, as is obvious (because of symmetry), the preference ordering for society is BAC.[10]

The same information used to generate an outcome under STV[11] or under the Chamberlin and Courant procedure—namely a complete rank ordering of alternatives—may be used to generate a faithfully representative committee by treating each potential representative as "representing" a given preference order and assigning weights in such a way as to faithfully represent the voters. Also, because faithful representation is just that, if voters are asked to submit a rank ordering of alternatives to be used for purposes of generating a faithfully representative committee, they have no strong reason to lie about their true preferences, even if they were thinking about sophisticated voting in the sense of Farquharson (1969), since the sequence in which alternatives will eventually be paired is unknown.

If there are $m(m - 1)/2$ independent representative orderings, then there will always be a faithfully representative committee. If there are more than $m(m - 1)/2$ independent orderings in the electorate, then there will always be multiple ways to faithfully represent the voters; if there are multiple faithfully representative committees, then we suggest that the one whose frequencies of preference orderings are most highly correlated with those in the population should be preferred. In any case, we can produce a committee that is at least as representative as that which will be produced by any other voting procedures.

It should be noted that faithfully representative committees can have very reasonable size, especially in comparison to the size of populations and the number of distinct orderings possible. We have indicated that faithful representation does not depend upon the size of the population; for m alternatives, faithful representation can be assured with a maximum of $m(m - 1)/2$ weighted representatives, and frequently with fewer than that. Table 1 shows how the size of the faithfully representative committees rises with the number of alternatives. Sometimes, however, we will be able to obtain faithful representation with less than $m(m - 1)/2$ committee members, as is shown in Table 2.

Table 2 supports our earlier claim that while every individual in a population the size of the United States could have a unique preference ordering over twelve alternatives, the population could be faithfully represented with a maximum of 66 representatives.

One inevitable problem in any form of representation is the possibility that the representatives will have to choose among alternatives that were not anticipated

Table 2. Maximal Size of a Faithfully Representative Committee

Preference Orderings and Committee Size	Number of Alternatives (m) 3	 6	 9	 12
Number of preference orderings ($m!$)	6	720	362,880	479,001,600
Number of single-peaked preference orderings (2^{m-1})	4	32	256	2,048
Maximal size of faithfully representative committee [$m(m-1)/2$]	3	15	36	66

when they were selected. This is, of course, a problem for any representative system, no matter how its representatives are chosen. Certainly, if the new alternatives have no consistent relationship to earlier ones, there can be no assurance that a committee chosen for its faithful representation over some given set of alternatives will faithfully represent the electorate in considering those new alternatives. However, most new issues are related to preexisting ones, and faithful representation may be preserved under some likely conditions.

One possibility is that the underlying choice continuum is preserved by the new alternatives such that each of the new alternatives fits along the continuum in a way that preserves the Net Margin Condition. If the committee members recognize the position of a new alternative along the continuum, and include it there among their own single-peaked preferences, the resulting vote margins cannot diverge from those in the electorate by more than the vote margins of the two preexisting bounding alternatives along the continuum. For example, if W fits in the continuum such that the group meets the Net Margin Condition for $ABCDWE$, then the margins for $m(A,W)$ will be between $m(A,D)$ and $m(A,E)$, and the margin $m(D,W)$ must be between $m(D,E)$ and $m(E,E) = 0$. Thus, if the representatives properly recognize the position of the new alternative along the continuum, they can continue to approximate faithful representation.

Another possibility is that preferences over the new alternatives will be related to preferences over the old alternatives in some (approximately) linear fashion. For example, if individuals who prefer A to B are more likely to prefer A to W than are individuals who prefer B to A, then, to the extent that the preferences over some new alternative are determined by preferences over preexisting alternatives, the margins by which that new alternative is preferred to each of the old ones will be linear combinations of the previous observed margins. If roughly the same relationships hold for all the committee members, then the faithfully representative committee will have the same margins as the electorate over the new alternatives, and continue to be faithfully representative.

If the set of alternatives used to select the faithfully representative committee reflects the range of issue dimensions about which future decisions will be made, then it is reasonable to expect that the committee may be able to represent the electorate faithfully on choices over new alternatives by processes such as those identified above. Furthermore, the problem of continued faithful representation when new issues arise is certainly no greater under a legislature/committee chosen for faithful representation than for any other system of representation. In particular, under procedures for faithful representation, voters have no more reason to lie about their true preferences than they would in a direct democracy.

Conclusion

This paper has laid out the basic characteristics of faithful representation and some considerations for systems designed to produce it.[12] Our intention has been to demonstrate the importance of faithful representation and its practicability in principle. Implementation would require further consideration of various potential problems, especially the determination of a particular set of alternatives upon which representation should be based. However, as noted previously, if faithful representation is guaranteed, then voters have no good reason to lie about their true preferences. Moreover, if we find a committee that will, for a given set of issues or issue dimensions, be faithfully representative of the voters in the sense we have identified, then we also have some reason to believe this committee will as a whole be in sufficient sympathy with the views of the electorate so as to be likely to arrive at decisions that are in substantial accord with societal (perhaps ideally informed) preferences, even on new items for which we could not have an a priori certainty of faithful representation.

We have shown that faithful representation can be achieved with committees of relatively small size, and that it is always possible to faithfully represent a group having any variety of preferences with a relatively small committee, all of whose members have single-peaked or single-troughed preferences with regard to any particular ordering we might specify. Furthermore, if the Net Margin Condition is satisfied, we can find a faithfully representative committee, all of whose members have single-peaked preferences. We suggest that the actual functioning of a democracy often approximates the latter situation.[13] Representatives are much more likely than the underlying electorate to have single-peaked preferences; yet, they can fairly represent the choices of the majority of the electorate, even though that electorate is not single peaked. Consequently, large electorates may have only slight tendencies towards single-peakedness, and yet be faithfully represented by their single-peaked representatives. Analysis of legislative voting finds most votes can be scaled along a single dimension (see, e.g., Poole and Daniels, 1985), while the candidate preferences of the electorates in 1980 showed that single-peaked individual preferences were relatively uncommon. We have shown how these apparent inconsistencies in the preferences of elites and masses can, in principle, nonetheless, be fully compatible with faithful representation.

Appendix: Proof of Theorem 5

THEOREM 5. *A group can be faithfully represented by a committee composed entirely of individuals with single-peaked preferences if and only if group preferences satisfy the Net Margin Condition. In particular for groups that satisfy the Net Margin Condition, a maximum of* $m(m - 1)/2 + 1$ *different weighted single-peaked preference orderings is sufficient to faithfully represent the group margins and the proportions of the total votes cast on each pairwise choice. One ordering less than that is needed to reproduce the margins without concern for proportions.*

Proof. Proof of the "only if" part of Theorem 5 is given in the text. The proof of the "if" part of the theorem consists of a procedure by which one can find a committee composed of $m(m - 1)/2 + 1$ single-peaked preference orderings that perfectly reproduce the margins and the total votes in the electorate. We can show that the margins alone can be faithfully reproduced with one less preference ordering.

Consider that the alternatives are ordered from l to m along the continuum.

Since the margins satisfy the Net Margin Condition, we know that if $N(i,j)$ is the number favoring j over k, and if $i < j < k$, then $N(i,j) \leqslant N(i,k) \leqslant N(j,k)$; i.e., within the half matrix above the diagonal, the numbers go up as one moves to the right and down the matrix. We shall pay no attention to entries on or below the diagonal.

We can proceed by accounting for each of the numbers in the matrix sequentially from the smallest to the largest. The smallest number is $N(1,2)$. We begin with exactly $N(1,2)$ of the preference ordering *1 2 3 . . . m*. Then, we subtract $N(1,2)$ from all of the rest of the numbers in the matrix, to obtain a new matrix. The next smallest number in that new matrix will be in the $N(1,3)$ cell. We use exactly that many of the *2 1 3 4 . . . m* preference ordering. Then we subtract $N(1,3)$ from all of the remaining cells to obtain a new matrix.

As we proceed at each step subtracting numbers from the cell entries in the preceding matrix, we obtain a new matrix with a sequence of zeroes on the left side of some rows above the main diagonal and with a sequence of nonzero entries, which we shall denote with Ys, on the right side of most rows above the main diagonal. We show below an example of such a matrix for a twelve-alternative voting situation:

$$\begin{array}{rcccccccccccc}
 & 1 & 2 & 3 & 4 & 5 & 6 & 7 & 8 & 9 & 10 & 11 & 12 \\
1 & X & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & Y & Y \\
2 & & X & 0 & 0 & 0 & 0 & 0 & 0 & Y & Y & Y & Y \\
3 & & & X & 0 & 0 & 0 & 0 & 0 & Y & Y & Y & Y \\
4 & & & & X & 0 & Y & Y & Y & Y & Y & Y & Y \\
5 & & & & & X & Y & Y & Y & Y & Y & Y & Y \\
6 & & & & & & X & Y & Y & Y & Y & Y & Y \\
7 & & & & & & & X & Y & Y & Y & Y & Y \\
8 & & & & & & & & X & Y & Y & Y & Y \\
9 & & & & & & & & & X & Y & Y & Y \\
10 & & & & & & & & & & X & Y & Y \\
11 & & & & & & & & & & & X & Y \\
12 & & & & & & & & & & & & X
\end{array}$$

In general, to find out at each subsequent step which preference ordering is to be used and with what weight, we identify at each step the *corner cell* or cells in the new matrix. A *corner cell* is one with zeroes above and to the left of it. In the matrix above the cells $(4,6)$, $(2,9)$, and $(1,11)$ are corners. If the coordinates of the corners are denoted by (i_1, j_1) for the one with the lowest j value, (i_2, j_2) for the corner with the next lowest j value, etc., then one may specify the preference ordering that should be added to the set as follows:

$$\begin{array}{ll}
j_1-1,\ j_1-2,\ \ldots\ i_1 & j_1,\ j_1+1,\ \ldots\ j_2-1, \\
i_1-1,\ i_2-2,\ \ldots\ i_2 & j_2,\ j_2+1,\ \ldots\ j_3-1 \\
i_2-1,\ i_2-2,\ \ldots\ i_3,\ \text{etc.} & j_3,\ j_3+1,\ \ldots\ j_4-1,\ \text{etc.}
\end{array}$$

In the example of the matrix above, we have the sequence *5,4; 6,7,8; 3,2; 9,10; 1,11,12,* as the preference ordering. It is clear that any such preference ordering is single peaked. Furthermore, it should be clear that this preference ordering contributes to the frequency of i_1 over j_1 and above, the frequency of i_2 over j_2 and above, the frequency of i_3 over j_3 and above, etc.; i.e., it contributes to exactly those nonzero cells that are yet to be accounted for and to no others.

One uses as many instances of this preference ordering as the lowest of the numbers in any of the corners; i.e., we give the preference ordering *54678329101112* the weight that is the lowest of the cell values in cells $(4,6)$, $(2,9)$, and $(1,11)$.

The procedure continues until all of the matrix entries are accounted for. This takes exactly $m(m - 1)/2$ preference orderings, because there are that many cells to account for.

Note that this is sufficient to reproduce all the numbers in the original matrix.

If one wanted also to reproduce the number of voters opposed, one would subtract the number of votes already accounted for from the size of the group and add in that many of the preference ordering $m, m - 1, \ldots 1$. That method serves to reproduce the exact number in favor and opposed in each pairwise choice, and so reproduces the desired proportions. Thus, to account for proportions we require $m(m - 1)/2 + 1$ weighted preferences. Q.E.D.

This procedure is not as complex as the abstract statement above might suggest.

A complete example with four alternatives can be used to illustrate the method: Consider the following matrix based on pairwise choices of 60 voters:

Alternatives	1	2	3	4
1	X	15	20	25
2		X	22	32
3			X	40
4				X

To create this matrix requires

frequency	*preference ordering*
15	*1 2 3 4*
5	*2 1 3 4*
2	*2 3 1 4*
3	*3 2 1 4*
7	*3 2 4 1*
8	*3 4 2 1*

and, since this accounts for only 40 votes, we also need 20 of the *4 3 2 1* preference ordering in order to total 60.

If one replaces the 15 orderings of *1 2 3 4* and the 20 orderings of *4 3 2 1* with 5 orderings of *4 3 2 1*, then the margins are faithfully reproduced, but the proportions are not. If one uses all seven different preference orderings, then both the margins and the proportions are perfectly reproduced. In general, if one were only interested in reproducing the margins, one could include either *1,2, . . . m* or *m,m*−1, . . . 1, whichever was weighted more, because these orderings are opposite and thus cancel each other out (e.g., 20 orderings of *1,2, . . . m* and 15 orderings of *m,m*−1, . . . 1 produce the same margins as 5 orderings of *1,2, . . . m* alone).

Notes

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1. They then go on to distinguish this view from one suggesting that representatives shall not reflect societal preferences, but what those preferences would be if every member of the society was "ideally informed, ideally expert, and ideally clear about his own interests" (Rogowski, 1981, cited in Chamberlin and Courant, 1983, p. 719). However, in either view "it is direct democracy (either actual or ideal) that is used as a measuring rod" (Chamberlin and Courant, 1983, p. 719).

2. In a multistage process of candidate selection (e.g., winner-take-all primaries or the U.S. electoral college), the failure to represent vote margins at each stage can result in the choice of a candidate who would not be selected by the (party) electorate as a whole. Indeed, in sequential decision processes, faithful representation at each stage is necessary to assure an even order of representation overall.

3. We use the term *faithful representation* differently than it is used elsewhere in the social choice literature (e.g., Murakami, 1968).

4. For a given ordering of alternatives—say *bcdae*—*single-peaked preferences* with respect to that ordering are those where each voter's utility reaches a peak at some given alternative and then declines as one moves left or right along the ordering away from that most preferred alternative. For example, if a voter had his or her peak at *c*, for the preferences to be single peaked we must have *cPd*, *dPb*, *cPa*, and *aPe*, where *P* denotes "is preferred to." Thus, for voters with peak at *c*, single-peaked preferences could be *cdbae*, *cdabe*, *cdaeb*, *cadeb*, *caedb*, and *cadbe*. In like manner, *single-troughed preferences* for a given ordering of alternatives along a line are those where each voter has a least-preferred alternative, and utility increases for that voter as one moves right or left along the line from that alternative.

5. Faithful representation does not imply there is a transitive social ordering, but merely that committee members make the same pairwise choices (and by the same margin) as the society as a whole. If societal majority preferences are intransitive, then the same will also be true for the majority preferences of any faithfully representative committee.

6. Alternatively, if we begin with the *1 2 3 4* ordering, we can get the requisite orderings by first moving the *1* successively further down to *2 1 3 4*, *2 3 1 4*, and *2 3 4 1*; then we move the *2* down to *3 2 4 1*, *3 4 2 1*; then the *3* is moved down to *4 3 2 1*. The first and last of these processes are the

same (only reversed), so the first is omitted in the final list of six preference orderings:

2 1 3 4, 2 3 1 4, 2 3 4 1, 3 2 4 1, 3 4 2 1, 4 3 2 1.

7. If a set of weights that reproduces the margins has fewer voters than are in the general population, then one can increase the votes in the committee to the number in the electorate simply by adding equal numbers of one of the preference orderings and its opposite; this adds to the total number of votes cast without changing any of the margins. However, if the committee has more votes than the electorate as a whole, there may be no way to reproduce the proportions without giving some preference orderings negative weights—one cannot substitute a positive weight for an opposite preference ordering in this situation.

8. We may have to use negative weights if the opposite preference ordering is not found in our society and our equational technique assigns a negative weight. However, since there are multiple preference sets to choose from, we usually expect to be able to find a solution with only positive coefficients.

9. A proof of sufficiency is obtained by combining results in Feld and Grofman (1985, 1986). For a counterexample to necessity, consider this eight-voter, three-alternative example:

5 *ABC*
2 *BCA*
1 *CAB*

This society has a transitive majority preference ordering of *ABC*. There is, however, no linear ordering for which the Net Margin Condition is satisfied. There are three possible cases:

ABC	*BAC*	*BCA*
$m(A,B) = 4$	$m(B,A) = 4$	$m(B,C) = 6$
$m(A,C) = 2$	$m(B,C) = 6$	$m(B,A) = -4$
$m(B,C) = 6$	$m(A,C) = 2$	$m(C,A) = -2$

As we see, in none of these cases is the Net Margin Condition satisfied.

10. In their article, Chamberlin and Courant (1983, p. 718) identify two key criteria for representation: (1) representativeness of deliberations, and (2) representativeness of decisions. Chamberlin and Courant (1983) offer a method for selecting representatives (and the weights that each representative is to have in deliberations that are to take place under a weighted voting rule) that is especially attentive to the first feature, representative deliberations. We have provided a method that is based on the second feature, representative decisions. Thus, our work and that of Chamberlin and Courant (1983) can be seen as complementary. However, if there is a conflict between representative deliberations and representative outcomes, we believe that faithful representation is considerably more important than symbolic representation of the sort that the Chamberlin-Courant method seeks to provide.

It is often possible to find committees that are arguably optional with respect to both criteria: representative deliberations and representative decisions. However, sometimes the two criteria will conflict—i.e., the intersection of the Chamberlin-Courant set and the set of faithfully representative committees will be empty. In the case of conflict we believe representative decisions ought to have priority.

We might also note that the Chamberlin and Courant (1983) definition of representative deliberations is not the only one that might be used. We could use as our litmus test the choice of a minimal *k*-cover for the set of voters (Harary and Cartwright, 1965). A *k*-cover is a committee such that, for every voter, some member of the committee is no worse than that voter's *k*th choice. Nelson Polsby (personal communication, October 10, 1985) has suggested that the members of the APSA Council be chosen in such a fashion that this group will constitute a *1*-cover for the political science profession. He has also attempted to formulate selection criteria for the council so as to achieve this end.

11. In STV elections, voters are required to rank order candidates. In some jurisdictions using STV (Wright, 1986), a full ballot must be cast for a ballot to be valid (i.e., a complete preference ordering must be given by each voter). Even if less than a full ballot is cast, a voter's indifference among alternatives that are not rank ordered (and their location at the bottom of his preference ordering) is not unreasonable to assume.

12. It would also be useful to consider various types of approximation to faithful representation, but we leave that task to a subsequent paper.

13. Recall that the Net Margin Condition may be satisfied even though most of the orderings in the group are not single peaked.

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POLICY MOTIVATION AND PARTY DIFFERENCES IN A DYNAMIC SPATIAL MODEL OF PARTY COMPETITION

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We present a model of party competition that produces more realistic patterns of results than those often emphasized in the literature. Reversing Downs (1957), we assume that parties win elections in order to formulate policies, rather than formulate policies in order to win elections. Voters are modeled first as having perfect information about candidate positions, and then under conditions of uncertainty. In simulation experiments we show that policy motivation and voter uncertainty can bring about persistent and predictable party differences in sequential majority rule elections. As the degree of voter certainty decreases, parties diverge towards their optima, whereas increases in voter certainty draw parties towards cycles in which party positions vary, but predictable issue stances are maintained on the average.

Why is it that there seem to be persistent and predictable differences between political parties? Are there incentives for parties to offer meaningful choices without demanding that at least one party insure defeat by moving too far from an opponent's position? We address these questions in the context of a simple model of two-party competition in two-dimensional issue space. Unlike many such models, which assume that parties maximize votes, our model assumes that parties have policy preferences of their own, and that they compete for electoral victory by virtue of adjustments away from their own optimum, or most preferred position. This policy motivation induces some persistent differences in party positions and platforms that might not exist if parties simply maximized votes, but the need to compete for swing voters limits the extent of those differences. In our models, party competition provides distinct choices for voters, and the nature of those choices is at least somewhat predictable.

Although persistent and predictable party differences are an element of conventional wisdom about politics, such a pattern is at odds with two of the central themes in the theoretical literature regarding party competition in issue space. One is that candidates of each party tend to converge on the position of the median voter if the space is unidimensional (Enelow and Hinich, 1984, ch. 2). The other is that under most conditions, there is no such equilibrium in multidimensional issue space (Plott, 1967). Indeed, a person with complete control over the agenda can cause majority rule outcomes to cycle all over the space and even outside of the Pareto set (McKelvey, 1976). As Romer and Rosenthal (1984, p. 472) observe, "The pervasiveness of rather large policy swings poses a puzzle for mathematical models of majoritarian processes. We have seen that these models predict either policy convergence or total policy instability, neither of which occurs in actuality."

In the more realistic setting of two-party competition, Kramer (1977) and McKelvey and Ordeshook (1976) have shown that outcomes are likely to converge to a limited subset of multidimensional issue space. However, these results still do not account for one of the most important stylized facts about electoral politics in the United States and elsewhere, namely, enduring differences between parties (Beck, 1982; Hibbs, 1977; Page, 1978; Tufte, 1978).

Most dynamic spatial models assume parties maximize votes, and this assumption contributes to the resulting predictions of convergence of policy choices in one dimension and instability of policy choices in more than one dimension. Of course, parties have historical identities and internal clienteles that they must please at the same time that they seek votes, and their movement in issue space may be constrained as a result. These identities and clienteles give parties a motivation to achieve policy goals, as well as to win office or maximize votes. Here we conceptualize these multiple motivations as a problem of expected utility maximization.

The model is first developed under the assumption of perfect voter information about the location of parties in issue space. It is then extended to conditions of voter uncertainty, and finally incorporates such nonspatial variables as the influence of incumbent performance and of other candidate characteristics, such as personality or perceived competence.

The Relevance of Existing Studies

A traditional assumption in spatial modeling has been that parties do not have a stake in policy beyond the bearing of policy choices on electoral prospects. In Downs' (1957, p. 28) words, "parties formulate policies in order to win elections, rather than win elections in order to formulate policies." If policies are chosen in order to maximize votes, parties might be willing to move anywhere in issue space, and a given cluster of voters might be appealed to by any party.

However, other work has recognized that party mobility in issue space is restricted, and that clienteles may have influence over positions taken by parties. For example, Aldrich (1983a, 1983b) extends the calculus of voting construct (Riker and Ordeshook, 1968) into a model of activist participation in parties. This explains how parties may be dominated by individuals with given policy preferences. Because party nominations are somewhat controlled by such activists, parties may have distinct policy identities.[1]

Following Coleman (1971, 1972) and Aranson and Ordeshook (1972), Wittman (1973, 1977, 1983) argues that candidates are motivated by policy goals as well as by the desire to win, and that election may well be a means to control policy rather than vice versa. Therefore, candidates might be expected to maximize expected utility, a function of expected policy outcomes as well as probability of winning (Wittman, 1973, p. 495; 1983, pp. 142–43).

Hansson and Stuart (1984) show that

politicians who maximize expected utility in this way over a one-dimensional issue space achieve a divergent equilibrium—that is, stable and predictable party differences.[2] Wittman (1983) derives such equilibria in higher dimensional spaces, using the assumption that parties maximize a "weighted mandate," or plurality-weighted utilities, rather than probability-weighted (i.e., expected) utilities. This choice, plus some rather strong concavity assumptions, allows him to prove the existence of equilibrium in his model, and then to make use of comparative static analyses. In his model, the actual policy resulting from the political process is assumed to be "a weighted average of the political positions taken by the candidates (the weights being the percentage of votes for each candidate)" (Wittman, 1983, pp. 143, 151). In making these assumptions, Wittman abandons an explicit majority voting framework as the process by which electoral results are achieved. The equilibrium result in the formal Wittman model therefore comes at the expense of neglecting to specify the process by which political decisions are made.

We prefer to return to the more conventional assumption of expected utility maximization and to a characterization of the electoral process as a series of majority rule elections. Although Wittman (1983, p. 143) asserts that "substitution of plurality for probability in the maximization of expected policy implementation has no effect on the mathematical properties of the model," we report some rather different results in this paper. Specifically, like many models of voting in multidimensional spaces, ours do not always generate equilibria. Moreover, the cycles that occur provide substantive implications about the nature of the electoral process.

Given problems of analytical tractability that result when the majority voting framework is retained, we have used numerical simulation techniques in our investigation. While simulation experiments do require the input of specific parameter values and do not yield general theorems, they can illustrate a range of possible outcomes and how outcomes change under alternative conditions. In contrast to Wittman, we find that equilibria may or may not exist in our models (as one would expect in voting models), and we are able to describe some circumstances under which equilibria are likely. Whether or not equilibria are found, persistent party differences occur, on average over time, under conditions of voter uncertainty as well as under conditions of perfect information. Generalizations of some of Wittman's intuitively plausible comparative static results also seem to hold up in our experiments, even when cycling occurs.

Our models provide a number of potentially important insights. They offer an explanation for persistent party differences in policies. This explanation is also consistent with occasional administration-specific aberrations, like those described in the empirical work of Beck (1982). In our model, these aberrations appear as parties try to lure away voters from the other party's natural constituency. The models provide an explanation for apparent "stability" in party positions, in contrast to much of the theoretical literature on spatial voting. Incumbent victories and dominant parties are also consistent with the results of our model (in contrast to the results of the traditional Downsian model). Our results also suggest the applicability of an "approximate" median voter result in multiple dimensions, when voters' optima lie only "approximately" along a single dimension. Finally, they suggest a relation between the sensitivity of voters to candidate policy differences and the extent to which parties will be able to impose extreme positions on the electorate, and the somewhat paradoxical result that cycling could be associated with more, rather than less, voter control over outcomes.

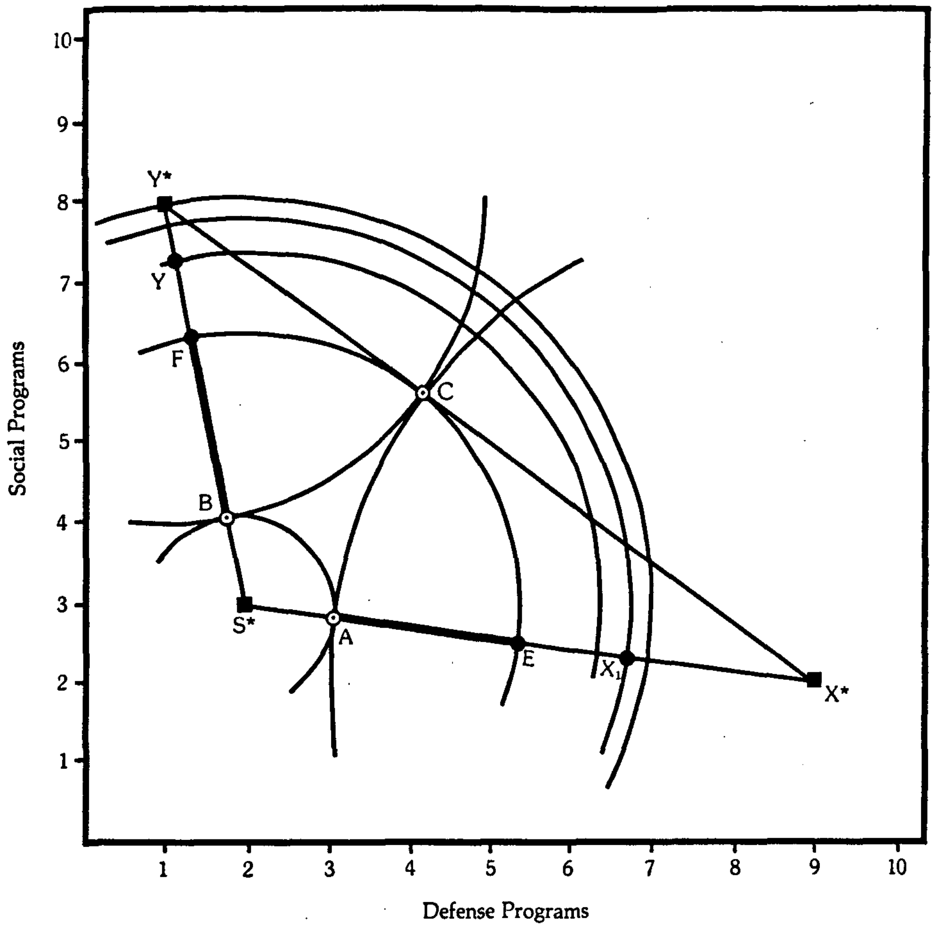
A Model with Perfect Information

In this section we discuss a simple spatial voting model in which each of two parties has ideological aims. We assume that voters are perfectly informed of party positions and vote for the party that would yield them higher utility.[3] We develop the argument first in terms of three voters in a two-dimensional policy space, where the policy issues are levels of spending for social programs and defense. Although our analysis is restricted to the three-voter case, this case is intended to be representative of the situation in which there are large numbers of voters divided into smaller numbers of distinct voting blocs. Voters are assumed to have Type I preferences—that is, each voter has a unique optimum point within the policy space that would yield maximum utility, and he or she ranks all other points in the policy space based on distance from the optimum. This implies that voters have circular indifference curves (there is an underlying budget constraint—voters will not prefer more of both goods when they must pay a share of the taxes required to finance additional purchases). When confronted with a choice between two parties offering differing platforms (i.e., positions in the policy space) voters will choose to vote for the party whose platform would yield the higher utility (or will abstain if the difference between parties is not larger than some very small finite distance).

In order to impose further structure, we make some assumptions about party behavior. First we assume that there exist two parties, X and Y, which compete to attain office in majority-rule elections. They do so by offering platforms to the voters. Following Downs (1957) and Kramer (1977), we assume that the incumbent party's choice of platform is constrained by its record. When they last won, the incumbents proposed a particular platform. In the meantime they have carried out that program and voters in this election attribute the same platform to them once again. Challengers are free to offer any opposing platform,[4] but here we do not assume that parties choose a program to maximize votes. Instead we assume that each party has a leader with preferences of his or her own, and that in each election the challenging party leader chooses a platform that results in a post-election policy closest to his or her optimum. Except in unusual circumstances, this also results in victory for the challenger.

For simplicity in the presentation, we will assume that two of the three voters have optima coinciding with those of the leaders of the two parties, X and Y. Those voters are respectively denoted V_X and V_Y. The third voter can be considered a swing voter, and is denoted V_S. Figure 1 illustrates optimal points for these three voters in the policy space: X^*, Y^*, and S^*. The axes are labelled *defense programs* and *social programs*, and units are arbitrarily defined so that quantities lie in an interval from 0 to 10. The illustration shows that V_X prefers a lot of defense and relatively limited social programs, while V_Y prefers less defense and more social programs. V_S takes moderate positions, preferring less of each than the most ardent advocate, but slightly more of each than the most ardent opponent. Significantly, his optimum does not lie on the line segment connecting the optima of V_X and V_Y. This line segment is the contract curve between voters V_X and V_Y, and the fact that V_S's optimum is not located on it implies that Plott's (1967) sufficient condition for a majority voting equilibrium will not be satisfied. Indeed, there will be no majority rule equilibrium—that is, no platform that can defeat all other platforms in pairwise majority votes. This

Figure 1. Two-Party Competition Under Perfect Information



■ Voter optima

⊙ Competitive solution points

$\overline{FB}$, $\overline{AE}$, C: set of policies occurring in cycles

is the typical result when the issue space cannot be represented in a single dimension.

We will now consider the policies proposed by the party leaders in a sequence of elections. To begin, we arbitrarily assume that in the first election, each party proposes its optimum as its platform. Since the swing voter, V_S, is closer to party *Y*'s optimum than to *X*'s, he votes for party *Y* along with V_Y, and party *Y* takes office. By assumption, in the next election party *Y* is constrained to retain its position, and the leader of party *X* will choose a winning policy to maximize his or her utility.

To see what this implies about the policy chosen by party *X*, we draw V_S's indifference curve through Y^*, V_Y's optimum, which is the prevailing policy. In order to be elected, party X must offer a policy that will induce V_S to defect from the party Y. To do so, while maximizing party utility, party X's leader will choose a point like point X_1, which lies on the contract curve between X^* and S^* just inside V_S's indifference curve through Y^*. By assumption, some small but finite movement closer to S is needed; if the movement were infinitessimally small, V_S would abstain. So party X is elected and policy X_1 is imposed.

Now consider V_S's indifference curve through X_1. In the next election, party Y's leader wishes to maximize party utility by selecting an appropriate policy. To do so, he chooses a point just inside V_S's indifference curve through X_1, but on the contract curve between Y^* and S^*. So point Y_1 is chosen, and party Y is elected.

A pattern has been established. In this example, leaders of the two parties are forced by competition to move from their own optima toward the optimum of the swing voter, along the contract curve between them. With each election, the winning policy is getting a little closer to V_S's optimum. However, that point is never actually reached. Suppose that, after a number of elections, party X wins by proposing point A, and that, in the following election, party Y wins by proposing $B + \epsilon_S$—that is, a point on the contract curve between V_Y and V_S that lies ϵ units beyond B in the direction of S.

Would party X respond in the next election by moving yet closer to V_S's optimum? Clearly not, since they could instead choose point C, which would yield the party higher utility, and induce V_Y, by tradition the party Y loyalist, to defect and vote for party X. Party Y could then defeat C by once again bringing V_S into the winning coalition by proposing $F + \epsilon_S$—that is, by moving to a point along the contract curve between V_S and V_Y that is ϵ units away from F toward V_S. Party X could then once again court the swing voter by choosing an appropriate point between E and A, and the cycle would repeat itself.

Essentially, an equilibrium cycle develops, and the set of policies in the cycle will all be located in small neighborhoods around line segments $\overline{EA}$ and $\overline{BF}$ and point C in the diagram. Policies for party X are most often located on segment $\overline{AE}$, while those for party Y are most often on $\overline{BF}$. Thus, pronounced party differences are observed, on average, even though parties occasionally shift policy dramatically (by going to point C) to attract a potential defecting opposition loyalist.[5]

Points A, B, and C are the points in "the competitive solution," a game theory construct that can provide a benchmark in our analysis (see McKelvey, Ordeshook, and Winer, 1978; and Miller, 1980; for a similar point without reference to the competitive solution, see Petry, 1980). The effort to win an election is seen here as an effort to form a winning coalition. Voter V_S is pivotal between two potential winning coalitions. The movement towards S^* is equivalent to a series of bargains that offer V_S more and more utility up to the limits offered at A and B, where V_S is indifferent between the two coalitions. Similarly, C is the point where each party loyalist is indifferent as between a coalition with the swing voter and a coalition with the opposition party loyalist. Each voter in a winning coalition is rewarded for supporting that coalition by a policy position just close enough to his or her ideal point to assure that he or she would not prefer another point in the competitive solution. Thus, the competitive solution has some appeal as a positive theory of coalition formation under majority voting, apart from the role it plays in the dynamic model we have developed above.[6]

What, then, are the important results emerging from this example? First, cycling occurs, but is limited to a rather small subset of the policy space that includes the competitive solution points, and is essentially repetitive. Second, persistent party differences in policy prevail on average. Third, despite the previous finding, occasionally a party leader will propose a policy designed to lure the other party's natural constituency, and in doing so must adopt policies that seem rather close to those typically offered by the opposition, at least on some issue(s).

Models with Voter Uncertainty

The model described above is limited by its simplicity and by its deterministic character. In the real world, voters do not have perfect information about candidates, and they make their decisions on the basis of considerations such as candidate appeal, retrospective evaluation of incumbent performance, and party loyalty, as well as on the basis of policy issues. We have modified the previous model in such a way as to approximate these features of reality.

The central change is the introduction of voter uncertainty about candidate issue positions.[7] The resulting stochastic model of electoral choice yields qualitatively different results regarding the series of positions taken by the candidates. Also, it provides a basis for introducing other, nonpolicy considerations and showing how they affect the pattern of results.

We continue to work with examples in which three voters and two party leaders have optima defined in a two-dimensional issue space. As before, voters' utilities are lower the farther away current policy is from their optimum. Now, however, we assume that as a result of voter uncertainty about candidates' positions, voters might not always vote for the party whose policy is closest to their optimum. Voters receive or collect bits of information that provide clues to party locations, but they can occasionally draw incorrect conclusions about which party is closest. The greater the difference in the distances from the voter's optimum to the locations of the two parties, the less likely it is that voters will be incorrect in their assessment of which party is closest. We therefore assume that the probability that voter *i* will vote for party *X* is a function of the difference in the distances from *i* to each of the parties. The closer party *X* comes to voter *i*'s optimum (holding party *Y*'s position fixed) the more likely the voter is to vote for party *X*. If party *X*'s policy is much closer to *i* than party *Y*'s, then the probability of *i* voting for *X* will approach one; if party *X*'s policy is much farther away from *i* than is party *Y*'s, then the probability of *i* voting for *X* will approach zero. In these models abstention is ruled out; if *i* does not vote for *X* he is assumed to vote for *Y*.

In our examples we use the following specific functional form to express the probability that voter *i* votes for party *X*:

$$P_{iX} = \frac{1}{1 + e^{\beta_i[D_i(x) - D_i(y)]}}, \qquad (1)$$

where

x is a vector giving the policy position of party *X*;

y is a vector giving the policy position of party *Y*;

β_i is a parameter to be described below; and

$D_i(z)$ is the distance to a given vector, here denoted *z*, from *i*'s most preferred point in the policy space.

This equation simply says that the probability that *i* votes for *X* is given by a logistic function of the difference in the distances to the parties. This specification embodies those properties described in the preceding paragraph.[8]

The parameter β_i measures the sensitivity of voter *i* to policy differences between the parties. If β_i is near zero, then voter *i* is not at all sensitive; that is, even if one party were much closer to i than the other, there would only be about a fifty-fifty chance of i voting for the closer party. When β_i is very large, voter i is almost certain to vote for the closer party, even when the party policies are quite similar.

Once again we assume a Downsian scenario of sequential elections, where incumbents are constrained to run on their records, but challengers are not constrained to do so. The motivation of challenging parties is again to attain favorable policy, and electoral victory is simply a means to that end. More specifically, the challenging party is assumed to minimize the expected post-election distance of the elected party's policy from the challenging party's optimum. This behavior is equivalent to maximizing expected utility, where the challenging party's position in issue space is the control variable.

In the case of party X challenging incumbent party Y, the problem is to:

$$\text{Minimize } L = P_X(x,y)\,[D_X(x)] + [1 - P_X(x,y)]\,[D_X(y)], \qquad (2)$$

where

$P_X(x,y)$ = the probability that X wins the election, given that party X takes position x and party Y takes position y;

and

$D_X(z)$ = the distance from party X's optimum to a given policy vector.

The probability P_X is derived using laws of probability given our earlier assumptions characterizing individual voter behavior. For example, for three-voter examples like those to be described

$$P_X(x,y) = P_{1X}(x,y)P_{2X}(x,y)[1 - P_{3X}(x,y)] + P_{1X}(x,y)P_{3X}(x,y)[(1 - P_{2X}(x,y)] + P_{2X}(x,y)P_{3X}(x,y)[1 - P_{1X}(x,y)] + P_{1X}(x,y)P_{2X}(x,y)P_{3X}(x,y), \qquad (3)$$

L turns out not to be a very tractable function. It is not globally convex in x, and can have multiple local minima. It is for these reasons that we perform simulations and solve models numerically instead of proceeding analytically.[9]

In this model, incumbents no longer necessarily lose; there will simply be some given probability of losing. However, if an incumbent is fortunate enough to win several successive elections, that does not change the challenger's optimal policy. When incumbents win, the same policies will be offered repeatedly until the challenger finally succeeds.

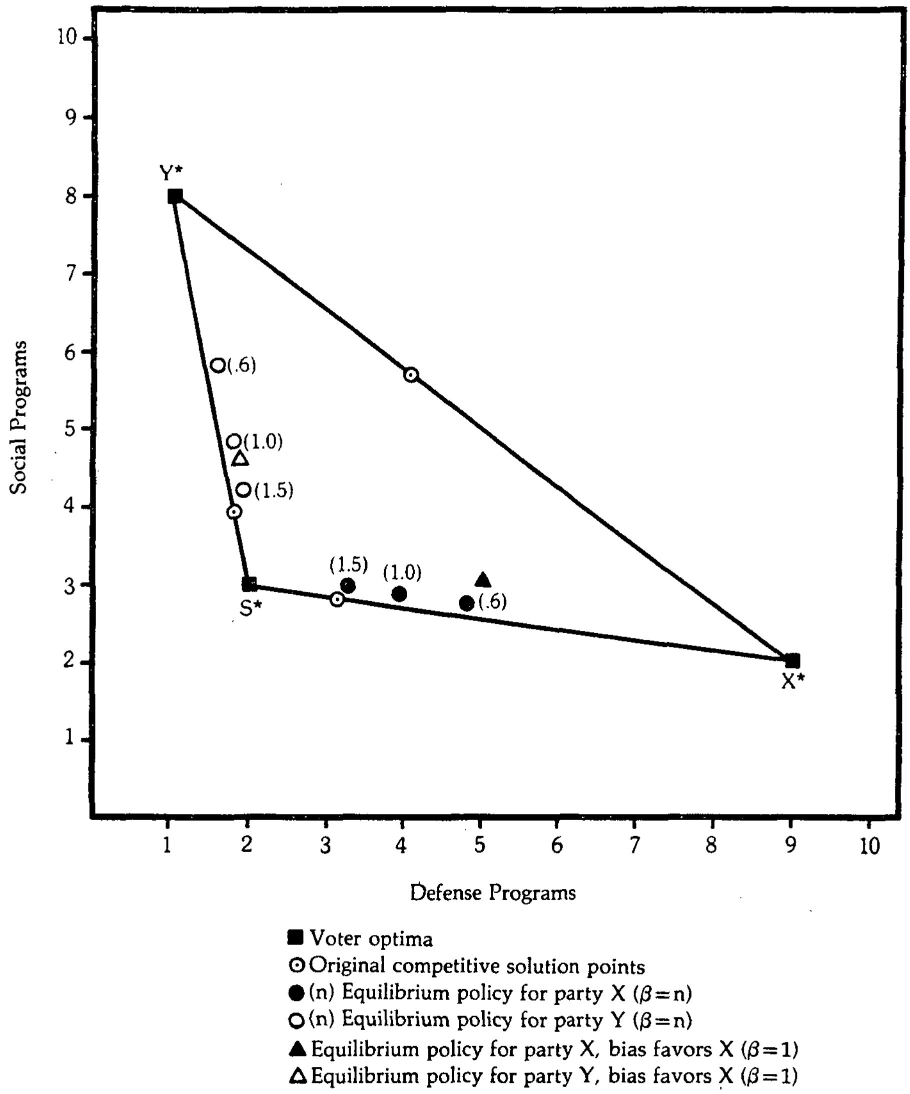
Since we are primarily interested in the sequence of policies that will be chosen, and since incumbents presumably will eventually lose, we install the challenger as the new incumbent after every election in our simulations. The reader should be aware that the reported sequences of policies should not be taken to imply that incumbents always lose, however. Repeated incumbent victories are possible, and we will show below that in some circumstances they are likely.

We are now ready to report on the outcomes of sequential policies resulting from electoral competition in simulations of the model described. In all experiments, the "sensitivity" parameters, β_i, are assumed to be identical for all voters, so the subscript is dropped hereafter. We have arbitrarily assumed that party X is initially the incumbent and have selected its leader's optimum as the initial prevailing policy.[10]

Varying Voter Sensitivity to Policy Differences

In the first experiment, we have applied the model of elections under voter uncertainty to exactly the same scenario described above in our model of elections under perfect information. Three voters, V_X, V_Y, and V_S, have the same preferences as before for combinations of defense and social programs, and one voter is located

Figure 2. Two-Party Competition Under Conditions of Voter Uncertainty



at the ideal point of each of the parties; β is set equal to one for each of the voters.

In this first experiment, parties sequentially proposed policies that moved them away from their respective optima toward that of the swing voter. The paths followed were close to the contract curves between party loyalists and the swing voter, but were actually located in the interior of the Pareto set. Policies did not reach the competitive solution points or begin cycling, however. Rather, the party leaders' policy paths converged to equilibrium points well short of points *A* and *B* in the competitive solution. The pair of equilibrium points for this experiment is designated by the label for $\beta = 1.0$ in Figure 2. Note that these points are also compatible with static equilibrium, in the sense that each party leader is satisfied with his policy choice there, given that his opponent is also at his equilibrium point. At the equilibrium point, the party whose optimum is closer to that of the swing voter has a slightly higher probability of winning.

We have performed a group of similar experiments replicating the one described above, but which imposed values for β ranging from 0.6 to 1.5. Recall that smaller values of β imply that voters are less sensitive to policy differences. In each of these experiments, the qualitative results were like those of the experiment just described: party policies moved toward the swing voter in successive elections, but converged at equilibrium points short of the competitive solution points.[11] Equilibrium points for these experiments are labelled for the appropriate value of β, and are superimposed on Figure 2. For higher values of β, the equilibrium pair is located closer to the competitive solution point. Thus, in this group of experiments, greater sensitivity of voters limits the ability of party leaders to impose their own preferences, and improves the welfare of the swing voter. It therefore follows that candidate ambiguity, to the extent it limits voter sensitivity, makes the parties better off in the long run at the expense of the swing voter. One might then view candidate ambiguity as a device that helps to achieve interparty collusion.

In Figure 3 we illustrate some results of an experiment in which voter sensitivity further increased, so that $\beta = 2.0$. In this case, a cyclical pattern similar to that discussed for the case of perfect information (i.e., perfectly sensitive voters) results. This is intuitively plausible. For $\beta = 0$, one would expect the party leaders to ignore the swing voters, who are completely insensitive to policy, and propose their own optima for policies. As β increases, parties must partially accommodate the swing voter, and the equilibrium points move toward the swing voter's optimum. When β is sufficiently large, and voters very sensitive, the results should approximate those for the perfect information case. The sequence of policies shown in Figure 3 illustrate a repetitive, or equilibrium, cycle that eventually results in this experiment. The equilibrium cycle may not be unique; a "mirror image" of the illustrated cycle could occur if an alternative starting point were chosen for the experiment. In other experiments with yet higher values for β, similar but more symmetrical cycles resulted. As in the perfect information model, party leaders occasionally adopt policies that may appear to be more appropriate for a candidate of the opposition party, but on average, parties' policies are closer to their own optima.

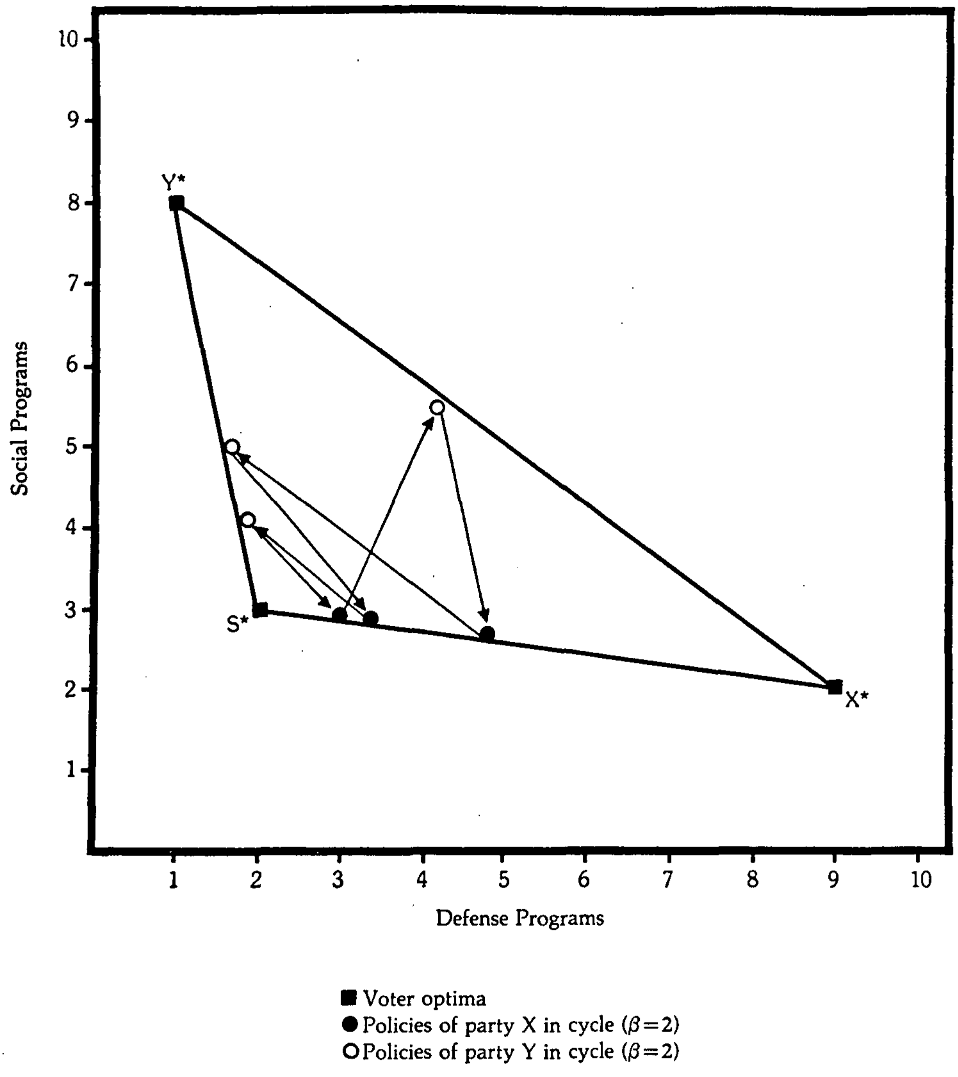
Since cycling and the paradox of voting, of which it is an example, have often been viewed as evidence of collective irrationality, it may seem ironic that greater sensitivity to issue differences leads to cycling. We will return to this issue in the conclusion.

Winning for Its Own Sake and Electoral Bias

In an alternative experiment, we have modified the model so that parties care about winning for its own sake, apart from the policy benefits winning may bring. In the previous experiments, winning an election was simply the way to come as close as possible to achieving policy goals. Now we incorporate the likely possibility that the motives involved in seeking office are still more complex, and that candidates are willing to make some policy concessions in order to win.

This change was accomplished by adding an additional constant utility loss of two units, to be suffered whenever the challenging party fails to win the election. For $\beta = 1$ and for the same voter ideal

Figure 3. A Cycle Induced by High Voter Sensitivity



points, the result was an equilibrium similar to that in the corresponding experiment in which there was no extra concern for winning. In this experiment, however, the equilibrium points were drawn slightly farther away from the parties' optima and into the interior of the Pareto set.

In the experiment just described, with $\beta = 1$, the extra desire to win did not induce a cycle. However, an extra concern for winning was sufficient to induce a cycle when β was set at 1.5 (recall that in the original group of experiments, $\beta = 1.5$ resulted in an equilibrium pair, i.e., no cycle). This indicates, as we would expect, that the added concern with victory is similar to greater voter sensitivity in making the model perform more like the perfect information case. Both phenomena draw party positions away from party optima and towards swing voters. Both phenomena lead to cycling if they are strong enough, because getting closer to the swing voters makes attracting defectors (and cycling) a more appealing strategy.

We have also run an experiment permitting voter bias. In this experiment, voters may favor one party over another, even if the parties are equidistant from the voter in spatial terms. This could result from retrospective evaluations of incumbent performance, such as those characterizing many recent studies of the effects of the economy on voting. Other sources of nonpolicy bias include party identification and responses to personalities or to demographic attributes.

Our original experiment was modified so that voter bias favors party *X*. Specifically, each voter now sees party *X* and party *Y* as equally desirable to him or her when party *X* is two units farther away from that voter than is party *Y*. With β set at 1.0, an equilibrium pair was again generated. One effect of the shift was to increase substantially the probability that the advantaged party would win each election. The bias had a limited effect on the position taken by the disadvantaged party, which moved only slightly towards the swing voter. The effect on the position taken by the advantaged party was much greater. Party *X* was able to move its position substantially towards its ideal point, while still enjoying an increased prospect of victory (see Figure 2).

Such a pattern is consistent with our expectations for a period of party dominance. The minority party may win from time to time, and the advantaged party does not have to move as far from its preferred policies in order to win as it would in the absence of bias. These results are also quite similar to those reported by Wittman (1983) in his analysis of voter bias.

Further Comparative Dynamic Experiments

When voters' optima shift, this will of course affect the policies offered by the competing parties, regardless of whether static equilibrium or cycling characterizes the initial state. We have, therefore, undertaken experiments in which a voter's optimum changes, and traced the sequence of policies that followed. One would of course suspect that if a voter's optimum moved in a particular direction, that policies in an equilibrium pair (or the set of policies in a repeated cycle) would tend to move in a similar direction. In several experiments we have undertaken, this is indeed the case. This reinforces the intuitively appealing comparative static results reported in Wittman (1983), and suggests that analogous results typically apply for the majority rule political mechanism we have described, even when there is no static equilibrium pair of strategies. We cannot claim that this is a general result,[12] but we have not yet encountered an exception in several experiments.

A special case of some interest involves the shift of the swing voter optimum toward the contract curve between the two party loyalists' optima. Such a shift implies that the distribution of voter optima more closely approach lying along a single dimension. In such a case, the resulting equilibrium points (or points in the repeated cycle) also lie closer to that single dimension. When voters are sensitive to policy differences, these points will be close to the swing (median) voter. Together these results suggest the applicability of an "approximate" median voter result; median voter theories will be useful predictors, even when the assumption of a single underlying dimension only holds as an approximation.

Larger Numbers of Voters

Our three-voter examples have helped simplify our analysis, while at the same time approximating the real world case in which there are small numbers of well-defined blocs of voters. In an additional set of experiments, we have considered larger numbers of voters whose most preferred points are scattered over the policy space. More specifically, voter optima were randomly generated from a uniform distribution covering the interval from 0 to 10 for each issue.

Computing exact probabilities of challenger victory is computationally burdensome when there are many voters. However a simple and accurate approximation is possible. First, as before, the probability that a particular voter will choose to vote for the challenger is generated using the previously defined logistic function, given the difference in the distances from the voter to the respective parties. Next, we compute the mean and variance for the random variable Z, which is defined to be the sum of the votes for the challenging party. Z will have mean Σp_i and variance $\Sigma[p_i(1 - p_i)]$, and, by a generalized version of the central limit theorem, is approximately normal for large numbers of voters (see Cramér, 1946, p. 213). The probability of a challenger victory (i.e., that Z exceed $N/2$, where N is the number of voters) is then simply found by retrieving the appropriate value from the normal distribution function.[13]

In other respects, the design of our experiments with many voters parallels those for the three-voter case with uncertainty. The most important features of these experiments are summarized below:

1. With 30 voters, and with the party optima located at (5,0) and (5,10), sequential strategies converged to equilibrium pairs in several experiments. The equilibria were in the vicinity of the center of the policy space; however, distinct and substantial party differences still appeared. Results of one of these experiments (with $\beta = 2.5$) are depicted in Figure 4. Results were similar in experiments assuming 100 or 500 voters.

2. Replicating the preceding experiment, but increasing β to 5.0, resulted in repetitive cycles that are also pictured in Figure 4. Within the cycles, strategies were limited to the central region of the policy space, and persistent party differences reflecting leader preferences occurred on average once again.

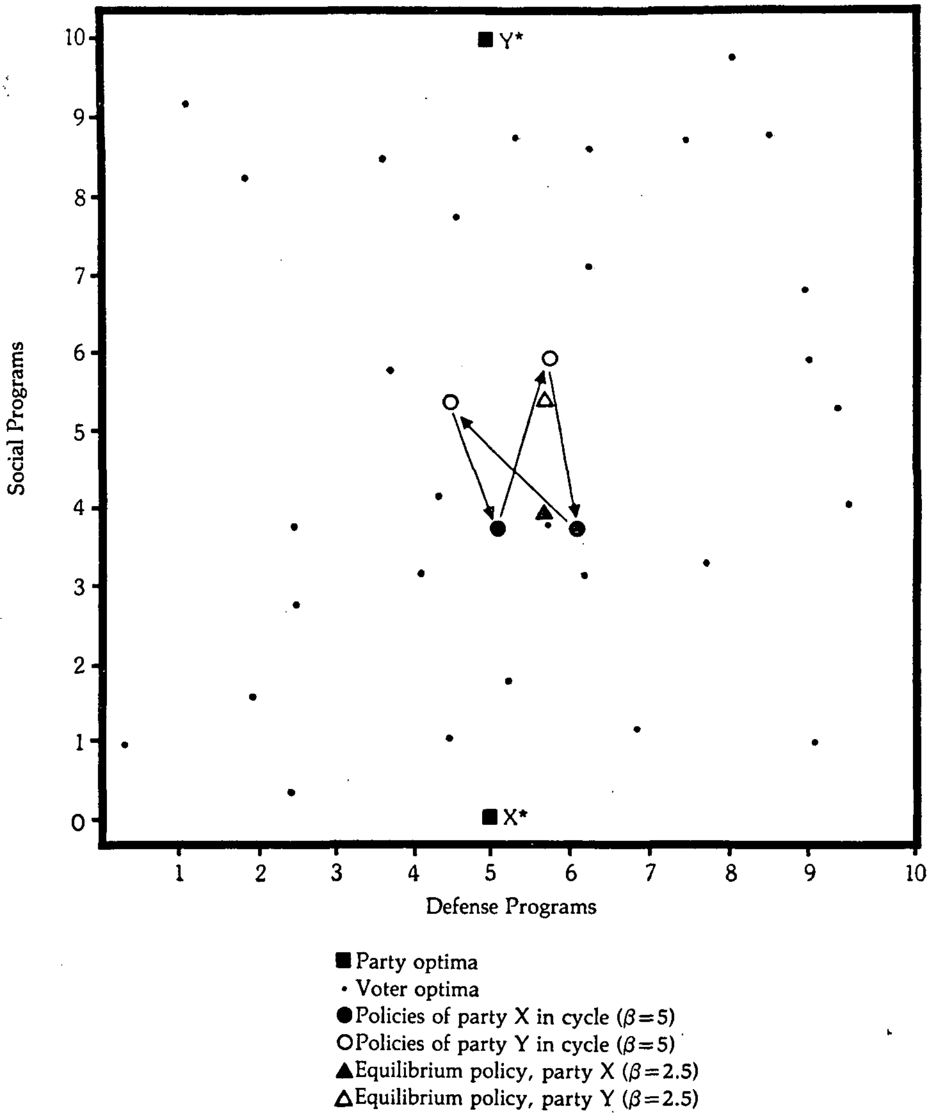
3. Comparative dynamic experiments yielded intuitively plausible results very similar to those reported earlier for the small numbers case. The effects of voter bias and a desire for winning in and of itself were also similar to analogous results for the three-voter examples.

Nonindependent Voter Decisions

Although the preceding experiments with modestly large numbers of voters result in persistent party differences, this may not be the case when the number of voters becomes extremely large. Arguments analogous to those of Tullock (1964) imply that for a very large number of voters, whose optima are distributed uniformly and whose voting decisions are independent of one another (as they are in our experiments above), party platforms would converge on a median position. Indeed, party positions were closer together for our 500-voter experiments than for our 30-voter experiments.

By extension, with millions of voters we might expect the party positions to be virtually indistinguishable. There are several reasons why a general version of our model may still predict party differences, even with very large numbers of voters. First, as we have already mentioned, large numbers of voters may not be uniformly scattered, but instead clustered in a few locations, like the

Figure 4. Two-Party Competition With a Larger Number of Voters



points in our three-voter example. Second, voter sensitivity to issue differences may not be uniform, as we have assumed, but rather increase with distance from the center of the issue space, resulting in greater polarization (see Rabinowitz, 1978, for an argument supporting this view). Similarly, if parties selectively inform voters about platform choices through advertising, they are most likely to inform those who will react favorably—that is, those located nearby. This is also likely to lead to greater sensitivity of voters located at the party extremes, and more polarization.

Here, we choose to focus on a third

reason for persistent party differences, even in the case of very large numbers. Contrary to the assumptions of our previous experiments, the decisions of all voters may not be independent of each other.[14] One illustration of possible effects on nonindependence comes from our previous experiments, if we suppose that the "voters" in those experiments are actually blocs of thousands of voters who always vote for the same candidate. Those results would then indicate party differences, even when there are many voters. Although real world voting blocs are not so cohesive, their impact will likely be to preserve the tendency for party differences, because they approximate the situation with smaller numbers of voters.

A lack of independence of voter decisions can enhance differences between policy-motivated parties in another way. For example, if party identification has an impact on voting apart from underlying spatial similarities, or if unanticipated events affect voters within particular groups in related ways, party differences can be enhanced.

Lack of independence of voter decisions can have the same effect, even if an unanticipated event moves all voters in the same direction. For example, a foolish statement or a strikingly poor performance in a presidential debate may move all voters in one direction. Since candidates are policy motivated, the uncertainty about who would be the beneficiary of such an unpredictable event leads them to take positions closer to their optima.

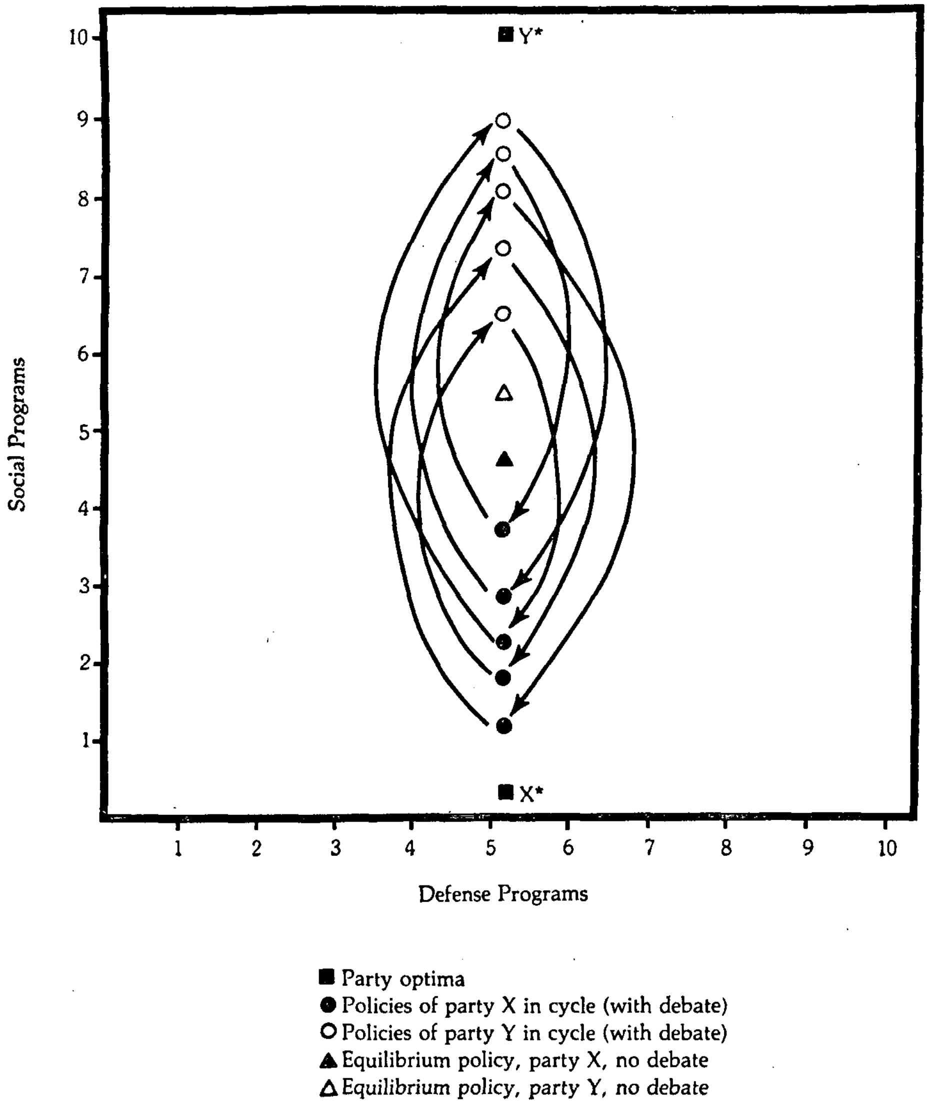
We illustrate this point with an additional experiment, using debate outcomes as a concrete example of a random event that may affect many voters in similar ways. In this experiment, we suppose that the two candidates choose platforms as before, but that after platforms are announced, the candidates will engage in a debate. The debate will influence voters, because it reveals evidence about candidate attributes (perhaps leadership attributes or ability to react to stress) that cannot easily be revealed through other campaign activities. Each candidate is assumed to have a 50% chance of "winning" the debate a priori. After the debate, each voter will vote as if he or she were one unit closer (in spatial distance) to the candidate who won the debate, reflecting one of the ways in which voters' decisions may not be independent.

As a consequence of this process, for any platform a challenger proposes, his or her predebate probability of winning the election is now computed as an average of his or her respective probabilities of victory under the two possible debate outcomes. Although predebate probabilities that voters will vote for a given candidate are not changed by adding the debate institution, the probability that a candidate will win the election given his or her own and the opponent's strategies generally is changed, because of the voter interdependence that has been introduced.

Failure to select a median policy no longer unduly risks defeat in the case of very large numbers of voters, since a candidate may win anyway if he or she is fortunate enough to win the debate. So long as there is an a priori chance of winning, candidates will wish to offer policies closer to their own optima than to the opponent's. Thus, party differences will persist on grounds such as these, even when many voters are evenly dispersed throughout the policy space.

Our experiments confirm this result for large finite numbers of voters. The introduction of debate effects does lead to larger party differences, and these differences do not appear to approach zero when there are very large numbers of voters. Interestingly, as Figure 5 illustrates, cycling can occur. In this experiment, in which 1521 voters were spread evenly over the policy space, cycling was limited to points on (or quite near) the line connecting the optima of the two parties, rather than all over the Pareto set, let

Figure 5. The Effect of a Debate



alone the entire issue space. Our previous findings that cycles are limited in scope and that party differences persist on average are preserved.

These results are of some independent interest with respect to the analysis of debates. Usually debates are viewed simply as a device for making the public aware of existing candidate differences. Our results show that debates not only reveal such differences, but may actually encourage them.

Conclusions

Two features distinguish our model of party competition in majority rule elections from much of the literature: policy motivation on the part of party leaders, and uncertainty on the part of voters. Both features are more realistic than the alternatives. Politicians do seem to care about policy as well as winning, and voters are uncertain about party positions. These features not only add realism; they also lead to results that are more plausible and familiar than those characterizing much of the literature.

The model is rich enough to permit variations in its features, and these produce associated patterns of outcomes that are observed in the political world. Politicians can vary in their balance of concern for policy and winning office, and voters vary in their sensitivity to party differences. Politicians can differ significantly in their attractiveness to voters on nonpolicy grounds, and therefore in their prospects of winning votes with given policy positions. All of these variations can be accommodated in the model, and produce sensible and plausible consequences, enhancing our confidence in the appropriateness of the basic approach. In all variants of the model, there remain persistent party differences, whether in the form of equilibrium pairs, or in the form of cycles that reflect enduring differences on the average.

The experiments show that parties can win while taking positions relatively close to their own optima when voters are not very sensitive to party differences. As voters become more sensitive to these differences, distinctions in party policy become smaller until outcomes begin to cycle, as they do in the perfect certainty case. Similarly, enhanced concern of parties with winning leads to breakdown in equilibrium and to cycling.

At first it may seem odd that enhanced voter sensitivity to issue differences and enhanced party concern with winning should contribute to the apparent "collective irrationality" of cycling. A priori, one might expect both to imply more voter control of collective decisions. It may also seem odd that less voter sensitivity to issues and less vote motivation by parties is associated with more clearly defined and distinct choices between parties. Clarity of choice has typically been associated with popular control, but here the relationship seems to be reversed.

However, as Nicholas Miller (1983) has pointed out, the cycling that seems irrational to the social choice theorist is very similar to the regular turnover of winners and losers that pluralist theorists have praised. What may seem like collective irrationality to the social choice theorist may contribute to the stability of the political system as explained by the pluralists. In any case, the cycling that exists in these models is recognizably similar to what is empirically observed, namely, moderate, but persistent and predictable party differences.

Notes

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1. A continuing issue in political science is whether, and under what circumstances, such activists are oriented more towards electoral victory, towards policy, or towards other personal benefits. See Schlesinger (1975) and Abramowitz and Stone (1984).

2. Calvert (1985) identifies conditions under which policy-motivated candidates will converge to a majority-rule equilibrium in a multidimensional issue space, when such an equilibrium exists.

3. These assumptions are identical to those employed by Wittman (1977).

4. So long as party leaders are included as relevant agents in defining the Pareto set, there is no agenda setter who would have a short-term incentive to propose an alternative outside that set. Therefore, any cycling we find will be limited to the Pareto set, unlike the possibilities identified by McKelvey (1976).

5. We have also analyzed several examples with more than three voters and have found qualitatively similar results.

6. The competitive solution points also have some normative appeal. This appeal derives from the rewarding of all members with a policy they find as desirable as any they could expect from another coalition, in effect giving each member his or her market value. The appeal derives also from the way the solution takes into consideration the desires of the excluded member. Winning coalitions that came closer to equal division between winning members would be less desirable to the losing minority.

7. We could also relax the assumption of perfect information on the part of candidates, but we think voter uncertainty about candidate positions is more central to the electoral process than the reverse, because candidates have more resources and incentives to gain accurate information about voters, and have strategic reasons to take ambiguous positions in order to enhance electoral prospects. See Page (1978, pp. 176–79).

8. It also implies that random variables indicating individuals' voting choices are statistically independent. Some effects of relaxing this assumption are considered below.

9. A FORTRAN program was employed to find the expected utility-maximizing policy strategies in sequences of elections.

10. Qualitative, and in some cases quantitative, results did not change when alternative starting assumptions were made.

11. Convergence to unique equilibria did not vary given alternative starting points for the sequential election process.

12. Given that equilibrium cycles need not be unique, it is conceivable that the intuitively appealing comparative static results could fail to generalize when there is cycling. However we have not found an example of this.

13. We are indebted to George Rabinowitz for suggesting to us this technique for increasing the number of voters in our experiments.

14. That is, the probability distribution functions for the random variables indicating the voters' decisions are unlikely to be statistically independent.

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ON THE STRUCTURE AND SEQUENCE OF ISSUE EVOLUTION

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How do political issues arise, and come to affect political party politics? We develop a theory and model of issue evolution, illustrating both by examining the dynamic evolution of the issue of racial desegregation. Our modeling concerns two central problems: (1) the structure of the evolution—a pattern of dynamic causality between the early policy cues from professional politicians, in Congress in the case at hand, and later mass response, and (2) the sequence of changes in elite behavior, changes in mass perceptions of party issue stances, changes in mass affect toward the parties, and changes in party identifications among citizens. We suggest that the causal process developed for the racial case is quite general for other times, other nations, and other issues. The theory of issue evolution is developed as a general statement of the organic connection between elite and mass behavior, a working model of the dynamics of American politics across time and issues.

Most issues most of the time lie dormant, stirring interest only in those especially informed and in those especially affected. They lend no weight to the color, tone, and meaning of partisan debate. They neither define party systems nor undergird party alignments. But occasionally issues rise from partisan obscurity and become so contentious, so partisan, and so long lasting that they come to define the party system in which they arise, to transform the grounds of debate which were their origin. This joint transformation of issues and party systems, which we call *issue evolution,* is realignment in the ordinary English usage of that term. It is a dynamic process resulting in the change of issue alignments. But the realignment concept has taken on such multiple and conflicting meanings from both popular and scientific usage as to make it an increasingly dubious vessel for the development of empirical theories of politics.

Mass party realignments, according to some theorists such as Schattschneider (1960), Sundquist (1983), and Riker (1982), may be interpreted as the redistribution of party support associated with the displacement of one political conflict by another. Viewed from this perspective, realignments are precipitated by the emergence of new issues about which the electorate has intense feelings that cut across,

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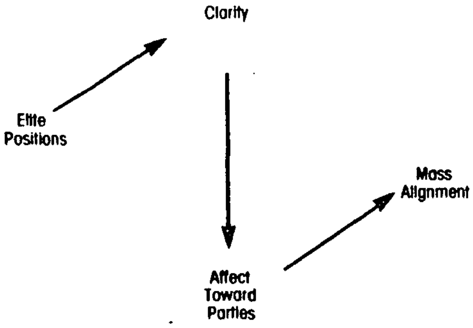
rather than reinforce, the existing line of cleavage between the parties. As the parties respond to the new issue dimension, they redefine the basis of the party cleavage with a new line of political conflict overlaying the old. Finally, a redistribution of partisan support occurs when the mass electorate responds to the new line of conflict represented in the party system.

This process may result in a new majority party. The new line of conflict may, alternatively, simply alter the coalitional structure of the parties. Whatever the ultimate systemic outcome of the process, its primary evidence at the level of the mass electorate is the increasing polarization between partisan supporters on the new issue dimension. But mass issue polarization is actually only the result—the most visible, cumulative effect—of a complex and multifaceted process. The causal nature of this underlying process, which we refer to as issue evolution, remains largely obscure and almost totally unexplored. What is the dynamic causal process that leads to mass policy realignment? Considerable light can be cast on this question, we believe, by examining the structure and sequence of issue evolution. First we outline a general causal model of the process, then look at specific evidence relating to its individual and separate elements, and finally conduct a more formal statistical analysis to uncover the dynamic causality inherent in the issue evolution process.

Figure 1. The Sequence of Issue Evolution



A Model of Issue Evolution

Most policy debate occurs among elected and appointed officials at the center of government; most attracts no significant public notice. When, occasionally, an issue moves from the limited "policy" environment to the larger stage of partisan politics, we naturally look for its partisan origin to those elite actors who framed the issue in partisan terms in the first instance. Figure 1 outlines the ensuing sequence of the issue evolution process. Following elite reorientations on contentious issues, comes a delayed, more inertial reaction in the mass electorate. When the elite polarization in progress is first confronted squarely by the mass electorate, the reaction often takes the general form of a *critical moment*. The critical moment is a mass polarization along the new line of issue cleavage large enough to be noticeable, but considerably less dramatic than the critical election of traditional realignment theory. Partisan conversion and electoral mobilization are the causal mechanisms that produce such rapid change. Critical moments occur, we presume, with some frequency. The new linkages between issues and parties which these critical moments establish often lose electoral relevance as quickly as it was gained. But in some cases the critical moment becomes the signal event for a less dramatic, but more substantial secular redefinition of the issue bases of political life. This secular reorientation reflects the continuing recognition of the changed positions of the parties after the critical moment, and it is driven by normal population replacement.

Changes in elite partisan behavior do not lead directly to mass partisan response. Rather, two intervening steps are necessary to link elite policy shift to mass issue realignment. First, the mass public must alter its cognitive perceptions of the parties with respect to the new issue dimension. Taking its cues from elite partisan actors, the mass electorate must recognize a difference in the positions of the parties on the new issue. But even changed perceptions, by themselves, are not likely to lead to changes in mass issue alignment. For issues to move voters to change their partisan identifications at the critical moment and bias the recruitment of new identifiers thereafter, the issue must also invoke a strong emotional response.

Changing perceptions of the parties must carry with them a heavy dose of affection and disaffection for the parties if they are to weigh against the stubborn inertia of existing partisan identifications. The public must not only perceive a difference in party issue stands, but it must also care about this difference. Only when these two intervening conditions are met—clarified mass cognitive images of the parties and then polarized affection toward them—will issue redefinition among partisan elites lead to new policy alignments among the mass electorate.

Changes in the components of party image, moreover, should be temporally bounded between the elite policy reorientation that is the beginning of the process and mass issue alignment which is its end. Time ordering is critical. Redefinition of the link between issue and party, however tentative and perhaps even unintended it may be, is a process that must begin with elite actors. In an environment where many policy cues are given and most are ignored, the crucial role of the mass electorate is to choose to respond to some cues. Which set of actors—political leaders or mass public—is the most important element of the process is probably an unanswerable question; we shall not in any case attempt an answer. But political leaders are assigned priority in time.

The Evolution of Racial Desegregation

The issue dimension we have chosen to examine is racial desegregation. While numerous issues have crowded onto the political agenda since the New Deal and competed for public attention, political analysts have singled out the issue of race as having the greatest realigning potential (Sundquist, 1983; Petrocik, 1981; Carmines and Stimson, 1981). Race has deep symbolic meaning in American political history and has touched a raw nerve in the body politic. It has also been an issue on which the parties have taken relatively clear and distinct stands, at least since the mid-1960s. Finally, the issue has had a long political life cycle. It has been a recurring theme in American politics as long as there has been an American politics and conflict over race has been especially intense since the New Deal. For those reasons, if a significant issue evolution has taken place in contemporary American politics, it has most likely revolved around the issue of race.

We turn now to the evidence. It will be presented in two parts. First we will see the evidence for the fact of issue evolution. Because the case is made elsewhere (see Carmines and Stimson [1981, 1984, 1986] and Carmines, Renten, and Stimson [1984] for various pieces of the picture, and also Petrocik [1981] for similar conclusions from a different focus), we will be brief. This first section is used to introduce the time series components of the model one at a time. The point to be made here is a simple one: that something happened. Our analysis is accordingly simple. Making minimal assumptions of our data, we will, for each of our interparty difference series to come, present a simple before and after test for the hypothesis that 1964 represents a breaking point in all the series. The congressional series are annual; the others, with minor differences, are biennial.

Part two of the exposition is devoted to the structure and sequence of issue evolution, the central focus of this work. When we turn to the evidence on structure and sequence, we will shift tack to make stronger assumptions by treating all series as annual, to abandon the implicitly linear dynamics of the before and after *t* test and its arbitrary focus on a single change point, and thus to explore the limits of a refined analysis of causal dynamics.

The Evidence for Issue Evolution

We argue that visible changes in elite behavior serve to redefine party images, to affect emotional response to the parties, and ultimately to realign the constellation of voter issue attitudes and party identifications to reflect earlier changes among the elite. We require indicators of each of these concepts: elite behavioral change, party image, citizen emotional response, and identification/issue alignment.

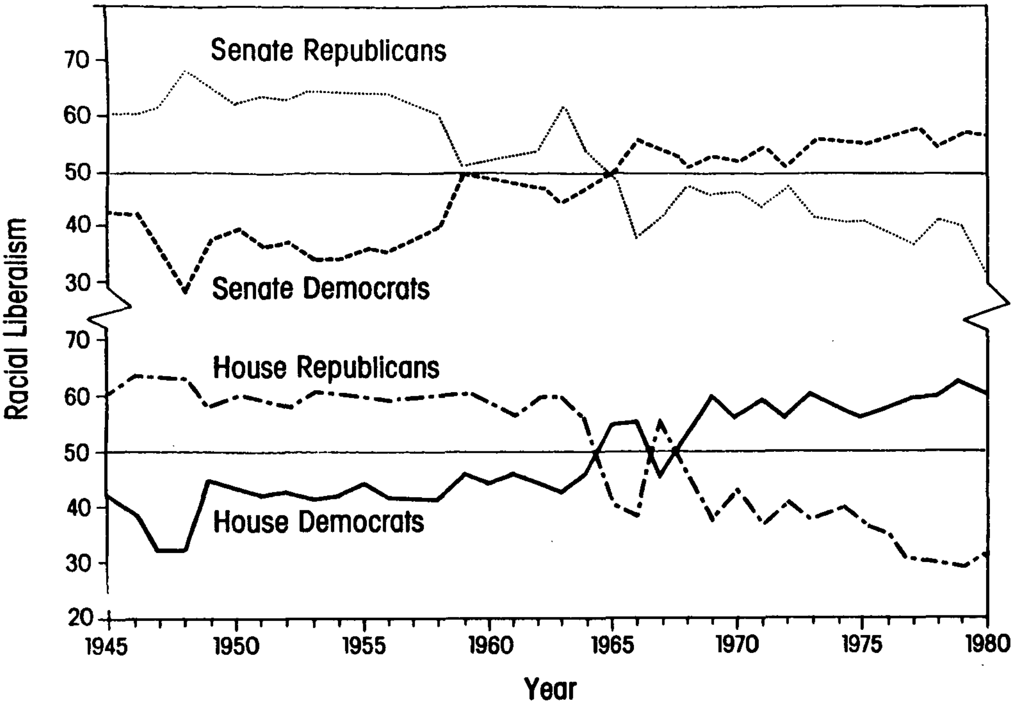
Party *elite behavior* is many things: acts of presidents, congresses, party officials, the parties in convention, and so forth. We have at one time or another looked at all of these, and each is arguably important in the issue case at hand (as would be the courts, except for their inability to lend partisan structure to issue conflicts). We choose to focus here on Congress, and specifically on roll call votes, because these frequent and public acts present a clean summary measure of what the parties truly stand for. While other elite actors, most particularly presidents, are undeniably important in reshaping party issue stances, none presents a regular pattern of behavior that is objectively and cleanly quantifiable. Our focus on congressional behavior should be seen not as an argument that Congress defines best what the parties support or oppose, but rather that it presents the best leverage for developing operational indicators of the messier whole of elite party behavior.

For the purpose of charting elite policy stands, we have used roll call votes cast in every session of both houses of Congress from 1945 to 1980. Following procedures developed by MacRae (1970) and Clausen (1973), we have developed standardized racial desegregation scales for all members of Congress, which are aggregated into party means for the following analysis.[1] These series represent the issue positions of the two parties on racial issues since the beginning of the postwar era.

Figure 2 presents the mean party positions on racial issues in the postwar era in the Senate and House. Examination of the figure reveals quite distinct and very strongly determined patterns of behavior. At the beginning of each series, the Republicans—then the "party of Lincoln" —were clearly the more moderate (and liberal in relative terms) of the two parties. Democrats were disproportionately southern in numbers, leadership, and image; the Democratic party had not yet developed its hard core of northern liberalism that would later become a counterweight to Southern influence, and still later dominate it. By the end of the series the parties had reversed their positions; Democrats were not only considerably more liberal in the aggregate, they were more liberal in all regions. Indeed, even southern Democrats are now less conservative on matters of race than the Republican party as a whole.

When we look at the differences between party positions over time (indicated by simply subtracting the Republican from Democratic mean positions for each year), striking patterns of racial issue evolution are in evidence. Beginning in the late-1950s the behavior of the parties tracks an unmistakable dynamic evolution toward a fundamental redefinition of the grounds of issue cleavage. Three significant movements in the Senate series

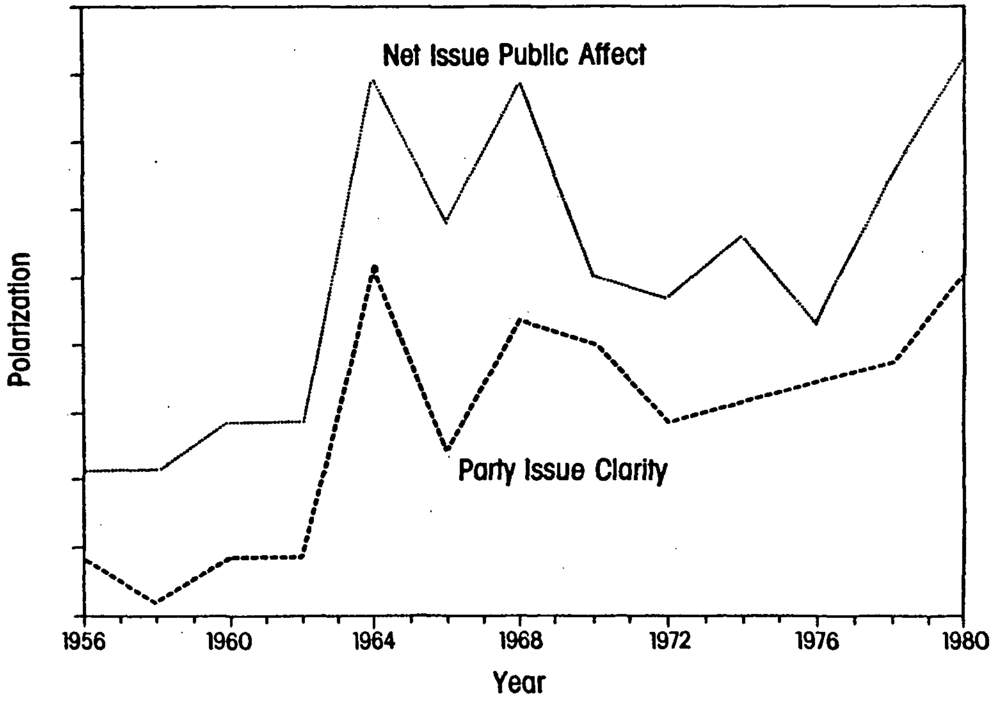
Figure 2. Desegregation Liberalism in the Senate and House of Representatives, 1945 to 1980



(see Figure 2)—sharp movements in 1959 and 1965, and a gradual growth process beginning around 1970—contribute to the ultimate redefinition of party stands. The House series mirrors the 1959 Senate shifts on a smaller scale and then begins a continuing dynamic growth process in 1965. The 1959 through 1963 movements in both houses are not a new polarization over race, but are movement toward the erosion of the old pattern of greater Republican liberalism, a necessary precursor to new polarization. The politics of the time may be seen as the beginnings of assertion by northern Democrats of a new majority status in their party, a six-year struggle to control the direction of Democratic policy that culminated by 1964 in unquestioned liberal control. Earlier liberal attempts to pass civil rights legislation achieved only limited success at changing the law, but appear to have been highly successful in laying the groundwork for a new Democratic liberalism that would so dominate the later 1960s. All of our indicators will show significant issue evolution, but the congressional series are the most striking.

To establish an indicator of the public perception of the party stances we turn to the University of Michigan Center for Political Studies (CPS) biennial national election series for a data source. Specifically, we examine questions tapping respondent perceptions of where the parties stand on racial questions. The data on perceived party issue positions are in three different question formats spanning three periods, 1956–1958, 1960–1968, and 1970–1980. The most dramatic changes in

Figure 3. Polarizing Clarity of Party Racial Issue Stands and Issue Public Affect Toward the Two Parties



issue perceptions occur, fortunately, within the common 1960 to 1968 format. To insure comparability of data collected under varying formats, we reduced the level of measurement for all years to the categorical variable, "Democrats more liberal," "Republicans more liberal," or "no perceived difference." With suitable manipulation (see technical appendix), all three question formats can be reformulated to yield the three categories.

The measure of mass perceptions of party issue positions—to be called *Clarity* following Pomper (1972)—is the aggregate percentage of each survey sample declaring the Democrats more liberal on desegregation minus the similar percentage of those perceiving the reverse ordering. Peak values are attained when respondents both see the parties as different and achieve consensus about which is more liberal. Lack of consensus or perception of no difference, both of which are common before 1964, reduces the measure. We took considerable care to maintain the conceptual linkages between the early series and the later ones. The empirical evidence bears witness to our success. Figure 3 shows both consistent behavior in the series across format changes and impressive variation within common formats.

The voters of Figure 3 failed to distinguish between the two parties on racial grounds until just before 1964 (before/after $t = 5.5$; $n = 12$) as noted at the time by Converse, Clausen, and Miller (1965). As late as 1960 (and, from other series, probably through about mid-1963), voters saw no difference between the parties, responding 22.7%, 21.3% and 55.9%—Democrats, Republicans, and no difference, respectively—to a question asking which party "is more likely to see to it that Negroes get fair treatment in jobs and housing." That lags notably behind the congressional series which show clear signs of movement toward changing party positions following the 1958 elections. It is equally, if less obviously, the case that mass perceptions lag behind the steady party polarization of later years as well, catching up at intermittent opportunities (such as 1980) years after the congressional parties changed.

We have seen direct evidence of the changing racial images of the Democratic and Republican parties. Whether it matters we have not yet seen, for evolving mass party images by themselves are necessary but not sufficient to account for issue evolution. More than clarity of perception is required if evolving party issue positions are to cause systematic issue sorting among the party identifiers. The issue must matter. It must strike home with enough force to influence the emotional ties between citizens and parties. As a first approximation of such emotional links we look to the simple affections and disaffections citizens display toward the parties over time.

To measure *issue public affect* we turn again to the CPS series and pursue a two-step indirect strategy. It is intentionally indirect to sidestep the "rationalization" and "projection" phenomena (Brody and Page, 1972) that are likely to plague any respondent commentary on the links between policy, affect, and party. Respondents who hold distinctive positions on the desegregation issue are isolated in the first step. Then, the positive and negative feelings expressed for each of the parties—without regard to race—by these distinctive issue groups form a summary measure of net issue affect. Our indicators then tap whether racial liberals and conservatives have differential overall evaluations of the parties.

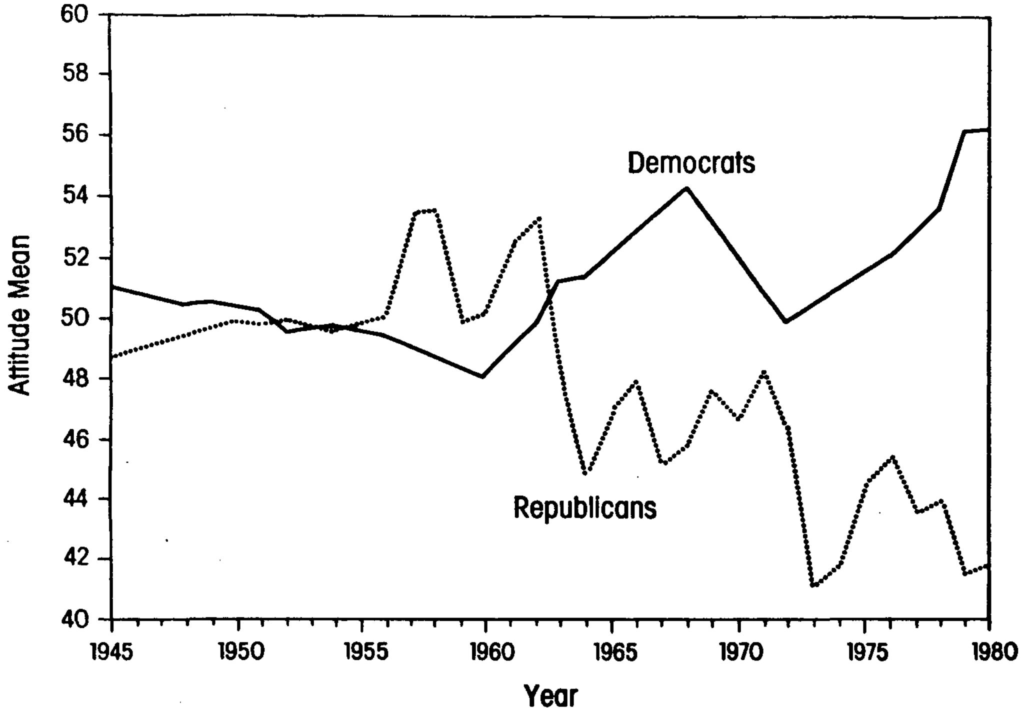
To isolate racial liberals and conservatives, we build a scale of desegregation liberalism for every CPS study where measures of both party affect and racial attitude are available (i.e., every presidential and off-year study from 1956 onward, excluding only 1958 and 1962).[2] Racial liberals and conservatives are then defined arbitrarily to be the highest and lowest quartiles on the scale. The middle quartiles are excluded on the rationale that "indecisives" on an issue have no grounds for emotional response to party position taking. Our goal here is to develop an aggregate measure of party affection and disaffection among racial issue publics. These relatively extreme quartiles are taken as operational indicators of "issue publics."

Affection and disaffection for the parties are tapped by the best available valence measures in each study. That, in general, entails the use of open-ended "likes and dislikes" about the parties for presidential studies and feeling thermometer ratings in off-year studies when the open-ended materials are unavailable. Both measures are scored in the Democratic direction—that is, positive scores indicate greater warmth for the Democrats than for the Republicans, and both are adjusted to a standard (50,25) metric for all years.

The net measure allows us to gauge whether citizens with distinctive issue positions reflect their issue biases in their emotional response to the two parties. Figure 3 shows a nonsignificant ordering of preferences before 1964. Racial issue publics—including blacks—liked the two parties about equally well in 1956 and 1960. Affect toward the two parties became clearly related to issue positions after 1964 (before/after $t = 3.4$; $n = 11$). And although the data are altogether independent of the cognitive images of Figure 3, the two patterns in the figure are suggestively similar, a matter we take up more formally below.

Systematic movements, something

Figure 4. Racial Liberalism of Democratic and Republican Party Identifiers



more than year to year fluctuation, would be expected to lead to something, and that is the final link in our analysis. The ultimate demonstration of the existence of an issue evolution is to show significant redistribution of public opinion on a policy issue among party identifiers. The new alignment of issues and party is the final result of the process of issue evolution, and the one that justifies the importance of all the others. It is the semi-permanent redefinition of the grounds of party issue conflict that gives evolving issues an importance considerably beyond the normal grist of electoral politics.[3] Its measure here is the simple interparty difference on desegregation issues, the mean position of all Democratic identifiers for a given year less the Republican mean.

Figure 4 displays the causal effect to be explained: the growing racial attitude polarization of the identifiers of the two parties. Figure 4 plots the desegregation attitudes of party identifiers from the SRC/CPS national election series for 1956, 1958, 1960, 1962, 1964, 1966, 1968, 1970, 1972, 1974, 1976, 1978, and 1980 (and from a Harris survey of November, 1963). Racial attitudes, an equally weighted summation of the survey items available in each cross section, are scaled with a common metric for all cross sections and reconstructed backward in time to create a continuous annual series.[4]

A sharp polarization along racial lines occurred in the turbulent 1960s (before/after 1964 $t = 5.3$; $n = 14$). Democrats became increasingly more liberal while Republicans became steadily more conservative, as some accounts suggest (see particularly Pomper [1972] and Converse, Clausen, and Miller [1965]). What is less expected—and hence more interesting—is that the polarization continued to grow during the 1970s when racial issues were no longer prominent on the political agenda.

A pattern of growth *following* the decay of the stimuli which created the initial polarization suggests simply that something else was going on. Polarization with a self-sustaining dynamic must be more than a response to visible "events." By postulating intervening processes that lag behind and then dynamically adjust to the reality produced by initial events, we will account for that self-sustaining dynamic.

To this point we have asserted causal connections between the varied components of issue evolution and presented evidence that is largely visual and intuitive. We turn now to a more formal analysis of our causal assertions, using transfer function analysis to uncover the dynamic causality within and between the various series.

The Evidence for Structure and Sequence

To examine the "sequence" in structure and sequence we will entertain dynamic formulations that allow the expression of causal effects between variables (series) in a manner that incorporates both empirical and a priori specification of delay, resistance, and dynamic adaptation. Our methodology is well developed in the time series literature, but sufficiently distinct from normal political science approaches that some words of explanation are in order. We will present our evidence fairly directly here, with little exposition of the statistical models and modeling techniques. Those matters are taken up in the attached appendix.

Estimating dynamic relationships asks much of our data, and we have chosen to push them to their limit. In some cases that is easily done. Our congressional series, based upon all the available roll call data for both houses of Congress for 36 years each, present an exceptionally clean look at the aggregate party behaviors. The survey data, on the other hand, are limited by the biennial structure of American elections and the questions survey institutions choose to ask. We employ 15 independent studies to construct our series for *Clarity*, what citizens think the parties stand for; *Affect*, how issue publics feel about the parties; and *Alignment*, the polarization of identifier issue attitudes. For *Alignment* (*AL*) we reconstruct 1945 through 1951 and odd-numbered years later (where independent surveys are not available) from recalled party identification of survey respondents. The perils of this technique are now well known (Niemi, Katz, and Newman, 1980). They are manageable here because for the period of SRC/CPS coverage, we rarely have to push recall beyond one year. The effect of errors of recall of party identification will be to introduce unwanted "noise" in the year-to-year changes of party identification/racial policy alignment (ΔAL).

Our intervening variables, *Clarity* (of issue perceptions) and *Affect* (of issue publics toward the parties), are measured biennially at best. For these variables we postulate no change between elections (i.e., $\Delta X = 0$) or before the first available surveys. If independent evidence for these cases existed, we would expect to see only sampling variation in the year-to-year change measures between elections. This amounts to the assumption that only election years matter in the evolution of links between issues and parties. The style of our analysis with these variables is much akin to intervention analysis (or to regression with dummy variables) where 0 and 1 values are postulated to stand for absent and present. The difference here is that in place of the 1s, we have empirical estimates of the direction and magnitude of change. The 0s indicate the absence of expected change. Again, if the assumption is false, the expected effect is conservative; a constant zero will neither be predicted by (when dependent) or aid in the prediction of (when independent) another continuously measured series of first differences. (In the appendix we report confirmatory [static] regression models based only upon the independent surveys where no such off-year assumptions are required.)

Estimation

We begin estimation by examining the dependent variable itself, mass party identification issue alignment (*AL*). This examination serves a statistical, not theoretical, purpose; it erects a benchmark for later explanations by accounting for the variation in *AL* which can be explained by the history of the series itself. To establish an explanatory benchmark we first fit a univariate noise model to the dependent series. As is traditional in Box-Jenkins approaches, we treat this noise model for our dependent series as a matter of no theoretical consequence, but a necessary prior step in order that later causal analyses will not be confounded by this source of extraneous variance. The *AL* raw series is nonstationary[5]—it does not fluctuate around a stable equilibrium—and thus we must model its first differences (Δ), the year-to-year changes that ultimately determine the level of the series.

A moving average process of the second order (MA[2]) fits the univariate *AL* noise series—that component of issue and identification alignment that is best accounted for by the history of the series itself. This is a sensible result given the two-year periodicity of American elections and of the election studies from which these data are constructed. The moving average process is marginally significant ($t = 1.8$) and produces a modest reduction in the residual mean square (a measure of unexplained variance) from 5.40 for the series modeled with no parameters to 4.97 for the Integrated Moving Average of the second order model. That latter value, the predictive error when the series is modeled entirely as a function of its own history, is a baseline against which the presumed causal explanations may be measured.[6] It is a conservative procedure in this case to treat the noise model as significant. That way the later causal explanations will not receive credit for accounting for this noise variance in the dependent series.

Elite to Mass Linkages

We introduce the Senate net party difference time series (S_t) in the transfer function analysis of Model 1 (Table 1).[7] Prewhitened cross correlations between the Senate and mass alignment series suggest an identification of the causal pattern between series that is in accord with what we expected to see. We know a priori (see Figure 5) that the first significant party movement toward new issue alignments in the Senate series is in 1959 (following the very large-scale replacement of racially liberal northern Republicans by liberal Democrats in the 1958 Senate elections) and that the first notable sign of changing mass racial issue alignment appears in 1963, when the Kennedy Administration first embraced the program of the Civil Rights Movement as its own.[8] Thus we have reason to suspect a four year lag between the two series.

Because both the Senate and mass alignment series evolve toward greater issue polarization, we could not be certain a priori whether a static or dynamic causal connection would exist between the two series. The data indicate a static one. Using the Senate series to predict mass party alignment produces a significant parameter ($\omega_0 = .23$, $t = 5.9$) and reduces the residual mean square on the order of 11% from the benchmark level.

Table 1. Explaining the Alignment of Racial Attitudes and Party Identification: Two Transfer Function Models

	Mass Issue Alignment is Dependent	
	Explanatory Variables Are:	
Variables and Fits	(1) Senate Alignment (Zero Order)	(2) Senate and House Alignment (Zero Order)
Senate Alignment[a] (Lagged 4 Periods)	.23 (t=5.9)	.12 (t=2.7)
House Alignment (Lagged 2 Periods)	—	.13 (t=3.3)
Moving Average (θ_2)	.94 (t=34.7)	.95 (t=25.9)
Measures of Fit		
Residual Sum of Squares	128.13	91.74
Degrees of Freedom	29	28
Residual Mean Square	4.42	3.28
Improvement Over Benchmark Residual Mean Square (4.97)	11.1%	34.0%
Autocorrelation		
Q[b] $\sim \chi^2$	5.9 (df=11)	13.0 (df=11)

Source: Annual time series computed by the authors from the 1952–1980 National Election Studies conducted by the University of Michigan Center for Political Studies, Harris Survey #1285, and from roll-call votes of the U.S. Senate and House of Representatives, 1945–1980.

[a] All series, independent and dependent, are first differences.

[b] Q is the Box-Ljung (1976) Q statistic for small samples, distributed as χ^2:

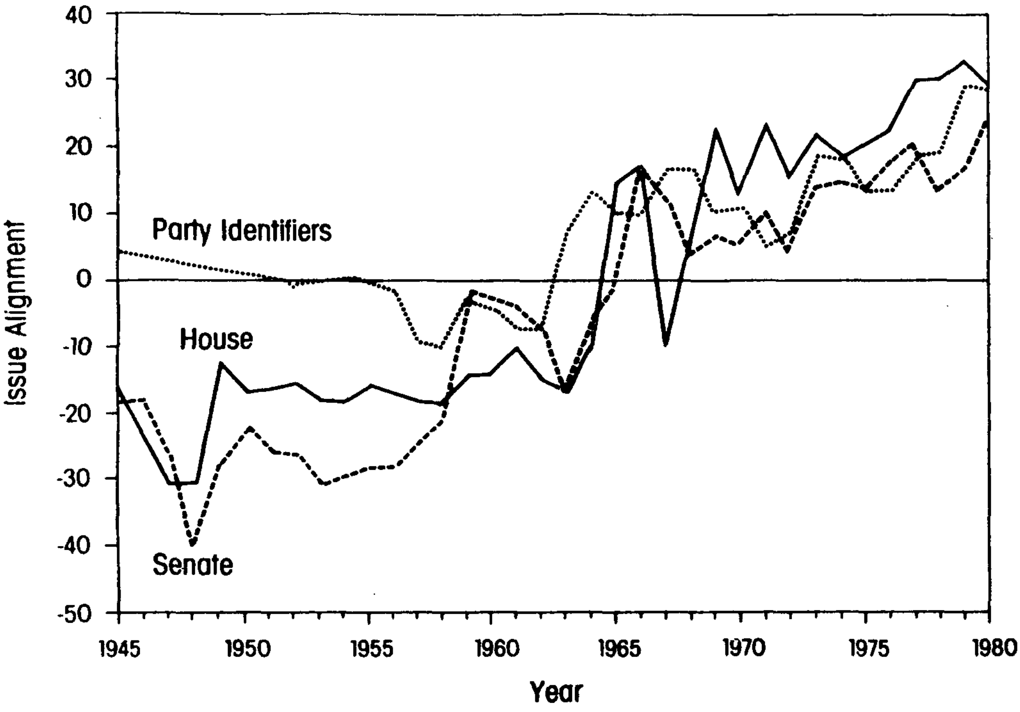
$$Q = (N)(N+2) \sum_{i=1}^{k} r_i^2/(N-i)$$

The form of the estimated model is linear because the postulated (nonlinear) dynamic adaptation process is already present in the independent Senate series. And since both track the same basic—S curve—dynamics, no additional between-series dynamic adaptation is present.

The House issue alignment time series (H_t) is introduced as an additional explanatory variable in Model 2 of Table 1 (see also Figure 5). While the raw Senate and House time series show much the same evolutionary pattern, the first differences in the two series (ΔH_t and ΔS_t) are only partially collinear (r = .28). The House series is much more inertial. It resisted most of the racial polarization that appeared in the Senate for some six years (1959 to 1965). But once the new alignment developed, it was more steadily maintained than in the Senate. Thus, the House offers a partially overlapping and partially different set of policy alignment cues that might have been publicly perceived.

Model 2 of Table 1 shows that the

Figure 5. Senate, House, and Party Identifier Alignments Together (Net, Democratic minus Republican Positions)



House and Senate series share about equally in explaining mass issue alignment. The combination of the two reduces the residual mean square by about one-fourth over that produced by the Senate alone, and some 34% over the benchmark level. Taken together, the two models of Table 1 offer evidence that elite party behavior, as manifested in our congressional indicators, may cause later mass issue alignment.

Establishing simple causality raises the question whether the *intervening* causal connections we have postulated are the correct ones. That is partly established by theory, for the intervening links are virtual logical requisites. The elite-to-mass issue alignment connection is fundamental. Empirical evidence of that linkage requires theoretical explication of plausible mechanisms which might translate change at one level into change at another—and perhaps in the process account for dampening and delay. Mass response to changing elite issue positions would seem to require reasonably accurate perceptions of those positions; and the polarization of affect in response to changes in party position requires clarity of issue perceptions.

Intervening Linkages

Working backward from mass alignment, we model the linkage between Affect and Alignment and then between Clarity and Alignment in the analyses of Table 2. Model 1 fits a first-order transfer function between Affect and Alignment. The resulting model produces the ex-

Table 2. Mass Issue Alignment as a Function of the Polarization of Party Affect and the Clarity of Party Positions

	Mass Issue Alignment is Dependent		
	Explanatory Variables Are:		
Variables and Fits	Affect Only	Clarity Only	Affect and Clarity
Affect			
δ_1	.27 (t=2.1)	—	.27 (t=2.0)
ω_0	.58 (t=7.3)	—	.59 (t=7.2)
Clarity			
δ_1	—	.35 (t=2.6)	—
ω_0	—	.19 (t=6.3)	.01 (t=0.4)
Moving Average (θ_2)	—	.56 (t=3.0)	—
Measures of Fit			
Residual Sum of Squares	69.77	71.67	69.38
Degrees of Freedom	33	32	32
Residual Mean Square	2.11	2.24	2.17
Improvement Over Benchmark Residual Mean Square (4.97)	57.5%	54.9%	56.4%
Autocorrelation			
$Q \sim \chi^2$	11.5 (df=11)	15.1 (df=11)	11.3 (df=11)

Source: Annual time series computed by authors from the 1952–1980 National Election Studies conducted by the University of Michigan Center for Political Studies, and Harris Survey #1285.

pected dynamic response—inertial resistance followed by adaptation—and a handsome improvement in our ability to account for alignment. The residual mean square, 2.11, from this estimation of but two parameters represents a 57% reduction in unexplained variance from the univariate benchmark. As an intervening link between elite party behavior (H_t and S_t) and mass alignment (AL_t), Affect should be more strongly associated with the dependent series and, by quite a margin, it is.

Following Pomper (1972) we postulate mass clarity of issue perception as a precondition of alignment. We further specify that the effect is indirect, through changing affect toward the parties among those who care deeply about an issue. That implies (*a*) a bivariate linkage between Clarity and Affect, and (*b*) a mediated—not direct—link between Clarity and Alignment. We address the simple effect of Clarity on Alignment in Model 2 of Table 2.

The form of the causal impact of Clarity on Alignment, a first-order transfer function, is similar to the Affect to Alignment relationship. Clarity differs from Affect by (1) having a considerably smaller initial impact, (2) joined with a larger dynamic adjustment, and (3) thereby leaving a smaller total effect. The Clarity estimation leaves systematic variation in the model residuals, requiring an additional parameter to model an MA(2) process. The Clarity to Alignment linkage is clearly significant, but slightly weaker than the Affect to Alignment relationship. It leaves more variance unaccounted for and requires an additional parameter in the process. The result is a marginally higher residual mean square.

The Clarity to Affect bivariate linkage is postulated to be instantaneous—both series are attitudes easily free to covary within our annual time interval—and thus can be estimated with a linear regression. The two series track one another quite closely even in their differenced form. The regression:

$$Affect_t = .57 + .40Clarity_t \qquad (1)$$

produces an R^2 of .89. Its slope is easily significant ($t = 16.8$) even in a small sample. Measured for aggregates—and therefore lacking the near automatic covariation from cognitive balance effects expected for individuals—and by very different techniques, the two components of party image retain nonetheless a striking empirical connection.

Whether Clarity works directly on mass alignment or is indirect through Affect is tested in Model 3 of Table 2. The result is unequivocal: the effect of Clarity on Alignment is entirely indirect, through Affect. Model 3 with both series in the estimation equation looks virtually identical to Model 1 (Affect only) with the addition of a substantively trivial and statistically nonsignificant parameter for the direct effect of Clarity on Alignment. That is reflected in a residual mean square higher than that produced by Model 1, a result of a lost degree of freedom without a compensating gain in explanatory power. The appropriate diagnostic evidence (the cross correlation function between prewhitened Clarity and the residuals of Model 3)[9] rules out misspecification as a possible explanation of this result; it shows no evidence of any connection between Clarity and the residuals at any positive or negative lag.

The linkage between elite behaviors, here the Senate series,[10] and Clarity can be modeled as a transfer function:

$$\Delta Clarity_t = .58\Delta S_{t-4} + (1 - .62B^2)a_t \qquad (2)$$

which demonstrates significant association ($t = 2.9$) between movements in U.S. Senate voting patterns and perception of party positions, with the same four-year lag we have seen before.

The final causal question we take up is the linkage between party elite behavior and mass alignment, controlling for Affect. Table 1 (showing the simple effect of elite behavior on Alignment) and Table 2 (showing the much stronger effect of the proximate Affect series) suggest that we are likely to observe no unmediated effect. Such is the message of the estimated model:

$$\Delta AL_t = \frac{.60}{(1 - .24B)}\ \Delta Affect_t - .02\Delta S_{t-4} + .04\Delta H_{t-2} + a_t. \qquad (3)$$

Again, it is virtually identical to Model 1 (Affect only) of Table 2, with the addition of two clearly nonsignificant parameters for the Senate ($t = -.5$) and House ($t = .3$) series. As would be expected, the addition of nonsignificant parameters costs more than it gains, leaving the residual mean square (2.44) notably higher than the Affect-only model. This evidence demonstrates that the intervening causal mechanisms are necessary to connect elite to mass response.

We are left then with evidence strongly in support of the issue evolution model of Figure 1. All of the hypothesized connections are significant. Party behavior causes mass perception of party positions, causing polarized affect toward the parties among issue publics, in turn leading to alignment along issue lines. None of the longer linkages is significant when modeled with intervening terms in the equation.

We have now traveled the path from observed changes in elite party behavior (the roll call series), to accurate mass perception of party position, to polarized emotional response to the parties based upon issue positions, and back to mass party alignment, which is where we began. We have traced the issue evolution process through each of its empirically distinct, but theoretically interdependent stages.

On Organic Theory and Inadvertent Recognition

We have explicated here a general model of issue evolution. While this model has been used to illuminate the evolution of racial desegregation, which is important in and of itself, our theoretical ambition is greater. We wish to explain, in general, how issue alignments and realignments are driven by mass response to the behavior of participants in national political institutions. Our empirical manifestation of issue evolution is race, but the theory should be equally applicable to other issues, past and future, which combine great salience and longevity with distinctive party movement. So too should it be applicable to other nations. Its assumptions about institutional cues and mass response are not specific to the American political context.

Conventional accounts of party realignment accord a fundamental role to mass electorates, treating institutional actors as responding to more central electoral forces. Such accounts provide a distorted picture of elite/mass dynamics. Tied to the normative democracy and the mechanistic metaphor of the U.S. Constitution, they require political professionals to have knowledge of issues—in both policy and political senses—that is *less* advanced than that of the amateur electorate. The origin of the policy dialogue between politicians and voters must lie, we believe, with the former, who provide definition to a multitude of issue conflicts.

The role of the electorate in issue evolution is to respond to some issues and not to others. The process is analogous to the natural selection of the biological world. Elites provide cues about issue definition. Many in number, complex, and contradictory, most are seeds on fallow ground, ignored by an inattentive electorate. The issue space—that tiny number of policy debates that can claim substantial attention both at the center of government and among the passive electorate—is strikingly limited by mass inattention. Alternative issues, or alternative definitions of the same issue, may be seen as competing for a portion of that space—a competition that is highly selective and often unpredictable.

Although elites lead—in the sense of acting first in time sequence—they neither control nor manipulate. However strategic their behavior in developing issue positions as levers to influence masses, the number of policy cues is so large and their effect so unknowable that the process takes on an appearance of randomness. The competition for issue space produces very large numbers of possible issue definitions of party politics. One such definition is occasionally selected when it happens to be well suited to the political environment of the moment. Like organic behavior generally, issue evolutions come to seem sensible, perhaps even inevitable, after the fact. They are almost unknowable before it.

Issue evolution produces representation as a by-product. But unlike the demand-compliance notions that dominate thinking about representative processes, this representation is inadvertent. It is systemic, not individual. It occurs without any single actor consciously attempting to produce it. Over the span of the desegregation issue, as should generally be the case, we can see evolution from a time when the party system was wholly unrepresentative—offering no coherent positions, no citizen choice—to the current pattern of issue-polarized parties, for better or for worse highly representative of their constituencies in the electorate. This representation is inadvertent because it was produced less by elite response to mass demands than by mass evolution toward existing elite positions.

American constitutional democracy is an exercise in eighteenth-century political mechanics. But the clockworks and balance wheels of its conception seem ill suited to explain its subsequent survival, development, and metamorphosis. For that we suggest that there is much to be gained from the organic thinking of a later century.

Appendix

Notes on First Order Dynamic Specifications

For many of the links in the model of Figure 1 we postulate first-order dynamics. What this entails is that a *change* in some variable X is followed (after a possible lag of k years) by a series of changes in the effect variable Y. In the model we will entertain, we expect a change in X (ΔX) at t to be followed after k lags by a perceptible change in ΔY (estimated by the parameter ω_0), and a continuing sequence of ever smaller changes in ΔY until it achieves an equilibrium adaptation to ΔX. (But before the equilibrium response to ΔX is achieved, Y is also responding to later innovations [both positive and negative] in X, ΔX_t, ΔX_{t+1}, ΔX_{t+2}, . . .) The decay in the sequence of changes is estimated by the parameter δ_1 (where $0 < \delta_1 < 1.0$) which distributes the causal effect of ΔX to a sequence of ΔY which, though mathematically infinite, in practice decays to a trivial level after a few periods:

$$\Delta Y_t = \frac{\omega_0}{(1 - \delta_1 B)} \Delta X_{t-k} + N_t. \qquad (4)$$

If we assume the lack of a systematic noise process in the model residuals—which we do here to simplify illustration, but do not do in the analysis—the model can be written in the more intuitively satisfying form 5:

$$\Delta Y = \delta_1 \Delta Y_{t-1} + \omega_0 \Delta X_{t-k} + a_t \qquad (5)$$

which expresses the current change in Y as a function of ΔX lagged k periods, and the previous ΔY, and a random disturbance a_t. Since the previous ΔY is also a function of its previous value and δ must be less than one, then the causal effect of changes in X must decay over time:

$$\Delta Y_t = \omega_0 \Delta X_{t-k} + \delta_1 \omega_0 \Delta X_{t-k-1} + \delta_1^2 \omega_0 \Delta X_{t-k-2} + \delta_1^3 \omega_0 \Delta X_{t-k-3} \ldots + a_t. \qquad (6)$$

Equation 6 accurately expresses change in Y only as a function of previous change in X—this is not a lagged endogenous formulation—and of random error. The sequence of exponentially decaying change in ΔY becomes growth when ΔY is cumulated back into Y, because it is the rate of change which is decaying.

The first-order model is an attractive conception of dynamic causality. It is likely to find application wherever inertia limits the responsiveness with which effect variables can adjust to changes in their causal environment, a very large class of problems. In the present instance, for example, party identification, if it is to deserve the special status it holds in theories of electoral behavior, cannot be understood as a labile response to the constellation of factors that cause it. The party identification of our theories is (at least in degree) a lifelong commitment, a standing decision, an ego identification, an ingrained loyalty, a habit, and an expression of solidarity with racial, religious, ethnic, and linguistic peers. The "identification" in party identification gives it a rootedness that makes it not impermeable to changes in the political world, but certainly highly resistant to them.

Policy conflict between the individuals and their respective parties (or attraction to the opposition) over matters of great material or symbolic importance may be understood to produce change in party identification. But the contest between ingrained loyalty and a short-term party attraction or revulsion is unequal; we expect loyalty nearly always to win out. But even as rock erodes under the force of a trickle of running water, sustained for a very long time, so party identification is likely to change in response to long sustained policy cues on matters of personal importance.

Two static approaches to the estimation of such dynamic causal relationships are common. Both are problematic. If we conceive of cause as an event in X at t, the effect of which is distributed over several later values of Y, we can approach the problem statistically by relating cumulative cause with cumulative effect, or by relating change in X with change in Y (perhaps with some lag k). The cumulative approach (e.g., relating the congressional racial time series to the party identification/issue alignment series) will produce stunning levels of apparent covariation, much of which invariably is spurious. If causal connection is present, the cumulative approach will find it, but with very little likelihood of identifying the correct functional form or direction. It is also likely to find it if it is not present. Cumulation—even of purely random variations—induces systematic behavior in time series, any two of which will have high levels of incidental covariation.

The problem of cumulation is the Type I error; it leads to the inference of cause when it is not in fact present. The examination of static relationships between change in X and change in Y has the opposite problem. If the effect of ΔX is distributed over several lags of ΔY, then even choosing the empirically optimal lag between X and Y will still—even with perfect measurement—understate the true relationship. If inertial drag limits responsiveness, then response time becomes stochastic, and we expect to see a distribution around some optimal value of k. No more than a fraction, and perhaps a very small one, of the effect of ΔX_t will appear in ΔY_{t+k}. If cause is not present, the method will not find it. It is also fairly unlikely to find it when it is present.

The first-order dynamic model is a middle course. It deals with variations in X and Y, not their cumulations, and hence avoids the spuriousness problem. It is in fact exceptionally rigorous against spurious covariation, an attractive property. But within the constraint of a parsimonious formulation, it also allows for a distributed effect of ΔX on ΔY that is both more realistic than a static formulation and much less vulnerable to Type II errors. It errs on balance toward the conservative side, because true causal effects which do not conform to the constraint of the first-order model do not count for the hypothesis.

Notes

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1. See Carmines and Stimson (1986) for a de-

Table A.1. Regression Respecification of Table 1

Variables	Mass Alignment (ΔAL) Dependent Senate Only	 Senate and House
ΔS_{t-4}: Senate (Lagged 4 years)	.15 (SE=.12)	$.23^{a}$ (SE=.13)
ΔH_{t-2}: House (Lagged 2 years)		$.21^{b}$ (SE=.14)
Constant	.64 (SE=1.20)	−.36 (SE=1.31)
N	13	13
R^2	.12	.28
$\bar{R}^2$	.04	.14

Source: Annual time series computed by authors from the 1952–1980 National Election Studies conducted by the University of Michigan Center for Political Studies, Harris Survey #1285, and from roll call votes of the U.S. Senate and House of Representatives, 1945–1980.

[a] $p < .05$ one-tailed test.

[b] $p < .10$ one-tailed test.

Table A.2. Regression Respecification of Table 2

	Mass Alignment (ΔAL) Dependent		
Variables	Affect Only	Clarity Only	Affect and Clarity
$\Delta Affect$	1.72* (SE=.33)		1.20* (SE=.58)
$\Delta Clarity$		.20* (SE=.05)	.08 (SE=.07)
Constant	.12 (SE=.67)	.47 (SE=.75)	.17 (SE=.67)
N	13	13	13
R^2	.72	.64	.75
$\bar{R}^2$	.69	.60	.69

Sources: Annual time series computed by authors from the 1952–1980 National Election Studies conducted by the University of Michigan Center for Political Studies, and Harris Survey #1285.

*$p < .05$ one-tailed test.

tailed analysis of the congressional series and for notes on scaling procedure.

2. Items and scales for the presidential studies are the same as those used in Carmines and Stimson (1981) and in Figure 2. For the congressional studies and the more recent 1980 study similar items are utilized.

3. Such issue redefinition by similar logic could be expected to lead also to issue dealignment, a prospect explored in detail in Carmines, McIver, and Stimson (1982), but well beyond the scope of this article. The evidence of that analysis suggests quite clearly that racial (and other policy) attitudes do predict individual movements away from party identification.

4. Each annual scale is composed of racial desegregation policy items that form the first principal component in analyses of all racial items. In general, the items deal with respondent preferences for federal desegregation policy (not including school busing, where the evidence suggests powerful influence of extraneous issue dimensions). The now-controversial reconstruction methodology and its specific application to reconstructing partisan racial attitudes are examined in detail in Carmines and Stimson (1984). That analysis confirms that the recall data are indeed problematic, as is reported "party identification" itself. We have limited the use of reconstruction methods to the period before 1956 and to odd-numbered years thereafter. One particularly crucial odd year, 1963, has been estimated from a Harris survey of November, 1963, completed (but not released) before the Kennedy assassination. See Munger (1977) for more detail on this well-timed exploration of public attitudes. Each of our analyses to come will be performed both on the annual time series, including those reconstructed, and independently on the shorter, mainly biennial series that require no use of reconstructed partisanship. The latter results, presented in Tables A.1 and A.2 in the Appendix, in every case confirm our interpretation of the former.

5. Limitations of space prevent us from presenting either the method of fitting transfer functions, the best source for which remains Box and Jenkins (1976), or the step by step modeling process of our application of the technique. Each of the models presented and discussed is a final estimation—the result of a lengthy sequence of preliminary and intermediary identifications, estimations, and diagnoses. Details of the step-by-step modeling are available in a technical appendix from the authors.

6. Our approach to causal analysis is the sometimes contentious notion of "Granger causality," that (in the simple recursive case) a series X_t may be said to (Granger) cause another series Y_t if the conditional expectation $E(Y_t|Y_t, X_t)$ produces superior predictions than an expectation based only upon the history of Y through $(t-1)$. Thus the univariate Autoregressive Integrated Moving Average (ARIMA) model for Alignment is the benchmark against which we judge predictive improvement. The residual mean square (the unexplained sum of squares divided by the degrees of freedom) is the criterion. See Freeman (1983) for a lucid far more comprehensive treatment of Granger causality in the context of international political economy.

7. On notation and terminology: Both "regular" transfer functions of the genre proposed by Box and Jenkins (1976), where one dynamic series exerts transfer causality on another, and the more common Box-Tiao (1975) intervention models, where the independent series is a dummy variable, are properly referred to as "transfer functions." But there are important differences in the identification and diagnosis of the two. Henceforth we shall use "transfer function" to mean regular transfer function and "intervention" to refer to the Box-Tiao, interrupted time series (Campbell and Stanley, 1963), or impact (McCleary and Hay, 1980) models.

8. These are only the most visible discontinuities in the series, and such evidence can be only suggestive of the proper lag structure. More generally, the model specifies that every change in ΔS is followed by a response in ΔAL four years later. When the changes take on a more continuous and subtle nature later in the series, connections become much more difficult to see, but the evidence suggests they are no less present.

9. This and other diagnostic evidence from the transfer function identifications and estimations are available in a technical appendix from the authors.

10. Either Senate or House series alone is a significant predictor of Clarity. But the small number of observations and the collinearity of the two series prevent a statistically reliable estimation of their joint effect. A similar analysis, not reported, shows mediated but not direct linkages between the elite series and Affect.

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WAR, ACCOMMODATION, AND VIOLENCE IN THE UNITED STATES, 1890–1970

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War, postwar demobilization, and economic depression are national crises that ultimately test the state's capacity to respond simultaneously to internal and external challenges. This analysis probes the nexus between crises and domestic violence, investigating how this relationship is mediated by the influence of two variables: the severity of crisis and the presence or absence of government accommodation. Box-Tiao impact assessment models are used to estimate the separate and combined effects of American involvements in wars (the Spanish-American War, World Wars I and II, and the Korean and Vietnam Wars), their postwar periods, and the 1930s depression on economic, social, and political forms of American violence from 1890 to 1970. After establishing historical evidence for the role of national accommodation, I demonstrate that strong, positive associations between severe crises and domestic violence are to be found during the tenure of nonaccommodating administrations. Accommodating governments are associated with either negative or historically weak linkages between severe crises and domestic violence. Overall, the evidence underscores the benefit of using broad theoretical perspectives for understanding the linkages between international and domestic conflict.

Students of political development have always recognized the contribution of environmental crises to the evolution of the state.[1] Crises are disruptive, episodic events challenging and promoting a state's capacity to respond to new problems. Their solutions often lead to the establishment of new institutional structures, powers, and precedents. The more extreme forms, war and major economic depression, are frequently represented as critical turning points in the state-building process. They are accused of undermining the established rules of resource distribution, altering the relationships between political elites and domestic contenders, and forcing state officials to take on new responsibilities in the face of class tensions and greater social complexity (Moore, 1966; Paige, 1975; Skocpol, 1979; Tilly, 1975; Trimberger, 1978; Wolf, 1969). In short, they have important cross-cutting effects by simultaneously magnifying the capacities of the state and escalating the risks of large-scale internal conflict.

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I have analyzed the three-sided relationship between national crises (war, postwar demobilization, and the 1930s depression), state accommodation, and the level of internal violence for the United States from 1890 to 1970. I contend that the observed linkages between crises and domestic conflict depend upon the position of governing elites vis-a-vis the claims advanced by internal political groups. On the one hand, governments willing to accommodate the demands of domestic political contenders generate weak associations between national crises and levels of internal violence. On the other hand, nonaccommodating governments, unconvinced of the potential merits of reforms, are more likely to rely on their coercive resources to suppress domestic challenges. In that event, the linkages between national crises and domestic conflict are expected to be strong and positively associated.

The hypotheses support the view that domestic violence is the byproduct of a sequence of actions and reactions between state officials and dominant and nondominant political groups. Since government officials have been cited as the primary agents of civil conflict, it follows that their behavior is a crucial intervening variable in the overall relationship between crises and domestic violence. Therefore, the theoretical task is to find out how state officials respond to internal challenges during difficult periods of national management, and to what extent their actions contribute to reducing or escalating internal violence. A brief review of the pertinent literature on the linkages between foreign and domestic conflicts will be discussed initially, in order to set up the subsequent theoretical and empirical analyses.

Previous Empirical Assessments

After reviewing 30 studies on the linkage between foreign and domestic conflict, Stohl (1980, pp. 312-13) finds only 5—Denton (1966), Denton and Phillips (1968), Flanigan and Fogelman (1970), Sorokin (1937), and Tanter (1969)—that systematically assess the specific relationship between involvements in war and large-scale internal violence.[2] Based solely on visual impressions of longitudinal data, 3 of these studies—Denton (1966), Denton and Phillips (1968), and Tanter (1969)—assert there is a linkage; the remaining 2—Flanigan and Fogelman (1970) and Sorokin (1937)—declare there is none. To complicate matters further, these studies do not overlap in their choice of samples, data, time periods, and temporal units of analysis. Together they present an excellent snapshot of the larger maze of empirical research in this area.

Theoretically, these five studies have been criticized for specifying overly simple relationships without regard for the role of intervening variables.[3] The strategy of reducing the question to ingroup/outgroup forms of conflict has made it all too easy to ignore factors such as political integration, repression, regime structure, and internal changes in the distribution of power.

Another problem is the implicit assumption that the theoretical linkages between external and internal conflict will always be positive, when there may be bidirectional relationships or "on-and-off" forms of associations. Despite the presence of demonstrable evidence for this notion, analysts have tended to either ignore it or construe it as an indication of no genuine relationship. Such reasoning, of course, points out the lack of consideration for intervening influences and the weak specification of previous theoretical frameworks.

The most sophisticated illustration of this flaw is Stohl's (1975, 1976) quasi-experimental analysis of American domestic violence between 1890 and 1970. As the most systematic empirical investigation of the effects of war on domestic violence, Stohl's studies eschew both the ingroup/outgroup propositions and the simple bivariate approaches of other analyses. Nevertheless, they postulate principles in favor of consistent unidirectional relationships. Specifically, Stohl hypothesizes that U.S. war involvements (the Spanish-American War, the two world wars, and the Korean and Vietnam wars) have influenced the mobilization of new domestic groups by enhancing their economic and social positions in American society. This mobilization in turn stimulates general demands for a redistribution of power and resources between lower and upper strata, and generates an escalation of domestic conflict.

Stohl's two studies depart from previous research by quantifying the frequency, duration, and intensity of conflictual events involving 20 or more persons, as listed in the *New York Times* and *New York Times Index.* A total of 2,861 domestic violent events have been identified for the years 1890-1906, 1913-1923, and 1935-1970. Using an interrupted time-series design and an ordinary least squares estimation, Stohl investigates whether nonrandom changes occur in these events at the start, during, and after U.S. war involvements. He finds step-level changes in civil violence both at the start of all five wars and in the postwar periods. However, no uniform pattern (in terms of statistical significance or positive and negative effects) holds for any of the five wars or for any of the dimensions of violence. Despite the unsupportive outcome, Stohl concludes the data generally support the contention that war escalates domestic political violence.

In contrast, I argue that the linkage between war (as well as other crises) and domestic violence is likely to be inconsistent. Whether it is positive, negative, or nonexistent ultimately hinges on government elites. As repositories of tremendous economic and political power, they have the capacity to influence the course and direction of domestic violence. Although wars can result in mobilizing new and old domestic contenders into the political process, this does not automatically have to end in violence. Elites can ignore or appease these groups in a variety of ways—in particular, compromise, reform, imprisonment, or execution, all of which may stimulate or diffuse domestic conflict. Even in cases where state officials are weak, their ineffective responses will have a substantial impact on civil conflict. Therefore, the following hypotheses will inextricably tie the behavior of state officials into the theoretical relationships between crises and domestic conflict.

Hypotheses

The Role of the State as an Intervening Variable

If war is classified as one form of national crisis, like postwar demobilization and depression, then the general question centers on how crises influence the political context in which government elites view internal challenges. Does the appearance of a crisis facilitate the development of flexible political environments wherein government elites make concessions to subordinate domestic groups? or does it sustain environments in which governments are determined to pursue status quo policies, regardless of public demands? Within either of these possible outcomes, the linkages between crisis and internal violence can be either positive, negative, or nonexistent.

For instance, crises inducing internal reforms may subsequently preempt or nullify internal violence. According to Block's (1977, 1981) and Skocpol's (1979, 1980) analyses of structural change in political systems, wars, postwar demobilization periods, and major depressions free governing elites from the typical constraints of dominant political groups.

Since they have a fundamental role in maintaining order and political peace, which crises jeopardize, state officials are apt to grant concessions, even though they may be at the expense of dominant groups. Such actions reflect the government's primary interest in extracting economic resources, recruiting military personnel, and diffusing potential violence during a difficult period of national management.

National crises can also transform political environments by producing elites who are willing to support reforms because of their motives to obtain elected office or other forms of domestic support. The Great Depression's effect on electoral realignment in the 1930s is a useful example (Ladd and Haley, 1978). It shows how the appearance of sympathetic elites in the White House, Congress, and the Democratic Party contributed to reforms in social security, labor, and farm policies.

In either event, the development of accommodating political regimes may account in one instance for negative or nonexistent linkages between crises and domestic violence. On the other hand, such regimes can be equally responsible for positive linkages if new political and social rights derived from recent concessions catalyze the mobilization of large numbers of people and economic resources into political organizations. With a new or renewed sense of efficacy and the perception that the government favors their claims, these groups may challenge other political elites not willing to embrace them (Tilly, 1978, pp. 145-46). Meanwhile, sympathetic officials, reluctant to take punitive measures against these emerging constituencies, are likely to resist the demands of dominant groups seeking repression. If the effect leads to direct confrontations between the contenders, the probability of conflict will increase and result in a positive association between crisis and domestic violence.

Since the internal effects of political reforms are difficult to foresee, governments may be conservative about undertaking major internal reforms when their positions in the international system are threatened. Instead, they may take repressive measures (e.g., deportations, arrests, censorship, and restrictions on public assemblies) against the leadership of opposition groups posing potential sources of challenge and instability. If the repression is consistently and effectively applied, officials will be able to wear down the position of domestic groups by making the costs of engaging in collective action too high (Tilly, 1978, pp. 101-6). In this case, the intervening effects of a nonaccommodating political environment can account for a negative or nonexistent relationship between crisis and internal violence.

However, if the government's coercive sanctions are weak and poorly directed, the effect may be one of intensifying antagonism and conflict among groups that fail to be inhibited by threats of retaliation (Gurr, 1970, pp. 239). The act of committing military forces abroad may weaken the government's capacity to suppress internal challenges. These circumstances could become revolutionary if domestic groups continued to press their claims by seeking the overthrow of the established order (Tilly, 1978, pp. 209-11).

Finally, circumstances may contribute to the rise of conservative policies after a time of political and economic reform; this is typical of postwar demobilization periods. During the war years, governments are able to expand the resources and powers of the state, and in the interest of preserving domestic peace may extend new economic resources and political concessions to internal groups. Yet during the postwar recovery period, those resources may contract—particularly if there is a recession—leaving officials with the responsibility of reconciling the commitments made to old and new domestic groups. In that event, they are faced with two choices: (1) increase the state's coercion against segments of the population in order to maximize the yield of resources for reallocation, or (2) break the commitments that will incite the least dangerous opposition. Either step is likely to lead to a defensive mobilization by internal groups and a subsequent escalation of domestic conflict (Tilly, 1978, p. 207). Although this generalization could apply to cases of victory or defeat, military defeats are apt to represent the worst cases of contraction. Moreover, this situation could describe major periods of economic depression.

All of these scenarios illustrate how domestic political regimes mediate the effects of national crises on the series of actions leading to internal violence. By understanding how crises act upon the political contexts and perceptions of governing elites, one can isolate the political processes that account for the levels of domestic violence, but the array of possible linkages complicates the problem of generalizing the directional relationships between war (or any other crisis) and domestic violence. It is difficult to make propositions when the variation in the intervening variable—namely, the presence or absence of government accommodation—can be responsible for the same empirical outcomes.

An alternative approach is to start with the presumption that crises stimulate the mobilization of domestic political contenders who decide to press their claims at a propitious moment. Since the claims advanced by these contenders are likely to be at odds with at least one other domestic group (not excluding government actors), the preconditions for internal violence are not only established but intensified in comparison with noncrisis periods. As central actors in the political system, state officials play a crucial role both in resolving these claims and in countering the effects of domestic instability. Whether they decide to repress these challenges through the coercive capacities of the state or to make concessions through government-sponsored reforms, the consequences are likely to result in some levels of domestic violence. The fundamental issue is the degree of severity. In other words, under what circumstances do crises generate levels of domestic violence atypical of previous noncrisis periods?

Nonaccommodating political regimes, it has been contended, are most liable to be associated with these kinds of severity, because of their greater propensity to rely on repression, which has been linked empirically with escalating civil conflict (Snyder and Tilly, 1972; Stohl, 1976). In contrast, reform-oriented regimes are less likely candidates, because they have a better chance of diffusing what could be serious quantities of internal violence by creating a political climate that disfavors attacks on domestic challengers. The actions of governing elites and the political context influencing the flexibility and range of potential policy responses are the keys to understanding the linkages between crisis and internal violence.

Therefore, the explicit hypotheses under investigation are formalized in the following statements:

1. The presence of an accommodating national administration will account for weak, insignificant linkages between crises and internal violence.
2. Given a context of nonaccommodation, there will be strong, substantial linkages between crises and internal violence.
3. Crises that sustain nonaccommodating political administrations will be associated with higher levels of repression than crises that induce reform-oriented regimes.

Given the variation in crisis attributes, it is impractical to assume all crises will have similar impacts on internal violence. However, the lack of available data and the inherent constraints in this kind of a study made it infeasible to isolate these effects and the role of accommodation at the same time.

Nevertheless, the additional influence of crisis intensity can be addressed in a roundabout way by distinguishing American war involvements as either *intensive* or *less intensive* affairs. *Intensive* wars are fought on a larger scale and result in the major expansion of new domestic institutions and state powers. Linkages between external and internal conflicts are expected to be strongest for these wars, because the foreign and domestic agendas will bring governing elites into more serious opposition with domestic groups over basic aspects of the economic, political, and social arrangements of their society. On the other hand, *less intensive* wars present fewer problems of domestic mobilization for governments; hence, they are expected to produce, at the most, weak conflict linkages.

If the two types of wars generate similar linkages, crisis intensity will no longer be an important conditional variable, and the role of accommodation becomes the central explanatory variable. In the event the less intensive wars engender consistently weak (or nonexistent) conflict linkages and the intensive wars yield mixed linkages, the subsequent analysis will concentrate on connecting the presence or absence of accommodation with the observed linkages of the intensive wars. Thus, crisis intensity will be held constant as the variation in the intervening variable is left to account for the fluctuations in the conflict linkages.

Operationalizing national accommodation is a relatively more difficult task in lieu of obvious quantitative measures. Alternatively, the appropriate historical records will be gleaned for evidence of reform activity undertaken by the executive, judicial, and legislative institutions of the U.S. government. Substantive reforms will indicate an accommodating national administration; their absence will denote nonaccommodation. Ultimately, the findings will comprise two components: (1) a qualitative analysis of national accommodation during the relevant U.S. political administrations, and (2) a quantitative assessment of the effects of war, postwar demobilization, and the 1930s depression on American domestic violence from 1890 to 1970.

Global versus Interstate Wars

Although identifying the most intensive wars is not a clear-cut process, one would expect them to be the most extensive—in terms of the geography encompassed and the number of major actors actively participating—and the most costly in terms of both lives and money. Furthermore, they would have the greatest historical consequences for internal changes in national expenditures, revenues, debt levels, the number of governmental agencies and personnel, and the general involvement of state intervention in everyday life. Recent studies consistently demonstrate an empirical distinction between interstate wars (the Anglo-American, Mexican, Spanish-American, Korean, and Vietnam wars) and global wars (World Wars I and II) regarding their short- and long-term influences on American state-building and processes of economic growth from 1792 to 1980 (Rasler and Thompson, 1983, 1985a, 1985b).[4] Global wars have statistically significant and much stronger permanent influences than interstate wars, which have temporary negligible impacts.[5] Other historical studies have also noted the growth in the United States' national administrative capacities, and the government's expanding role in resolving broad economic, political, and social issues during World Wars I and II and the Great Depression

(Brody, 1980; Hawley, 1979; Skocpol, 1980; Skowronek, 1982; Wilensky, 1975). Consequently, it is hypothesized that as the most serious crises, World Wars I and II, their postwar periods, and the 1930s depression will be associated with significant conflict linkages; the interstate wars and their postwar periods will have weaker, less significant associations.

Research Design

Data and Indicators

Fortunately, the presence of data on American violence in Stohl's (1976) analysis makes the following tests possible. However, the annual domestic violence data cover the periods 1890-1906, 1913-1923, and 1935-1970. The lack of a continuous time series requires slicing the data into prewar and postwar observations without strong theoretical guidelines. The outcome can seriously compromise the validity of empirical findings. For instance, the choice of 1935-1941 as the pre-World War II period, which bypasses the violence related to labor and unemployment in the early 1930s, may result in overestimating the level of violence from the prewar to postwar years.

Meanwhile, a separate analysis of the three data sets complicates making historical comparisons over time. The central question is not only how wars influence the level of internal violence, but whether the change in the level of the violence is truly different from all other nonwar years also characterized by excessive levels. The most effective way of escaping these problems is to employ data and methodological approaches that depend on long, sustained records of violence. Since Stohl's (1976) data collection procedures are outlined clearly, the task of expanding the annual data for the periods 1907-1912 and 1924-1934 is relatively straightforward. The present analysis will use an annual measure of the magnitude of domestic violence. The frequency of all violent events is multiplied by their duration to yield four time series under investigation: *economic, social, political,* and a combined measure of all three labelled *total violence.* Plots of these series are provided in Figures 1 and 2.

The decision to maintain Stohl's economic, social and political classifications is based on the view that conflict linkages may be contingent on the type of domestic challenge being made. As an example, the state's wartime interest in preserving economic stability for the production of war materials may lead to concessions for labor unions; at the same time, other groups considered to be unacceptable threats to the state (e.g., anarchists, International Workers of the World, and the Communist party) receive different forms of response—namely, repression. Such discrimination will inevitably affect the observed conflict linkages.

Stohl (1976) identifies economic-related violence by the types of issues (e.g., collective bargaining, wage increases, unemployment) at stake and the groups participating in the conflict (e.g., management versus labor; landlords versus tenants; farmers versus bankers; and agricultural laborers versus landowners). Social-related violence comprises racial, ethnic, religious, and educational disputes (e.g., whites versus blacks, Indians, and Orientals; native Americans versus foreign immigrants; and students or faculty versus university administrators). Finally, politically-related violence involves disagreements over governmental policies or ideological differences (e.g., Republicans versus Democrats; right- versus left-wing groups; government actors versus antiwar protestors; Communists versus Socialists; and government actors versus Communists and Socialists).

In addition to these dimensions, Stohl (1976) distinguishes among pro-system

Figure 1. U.S. Economic and Political Violence, 1890–1970

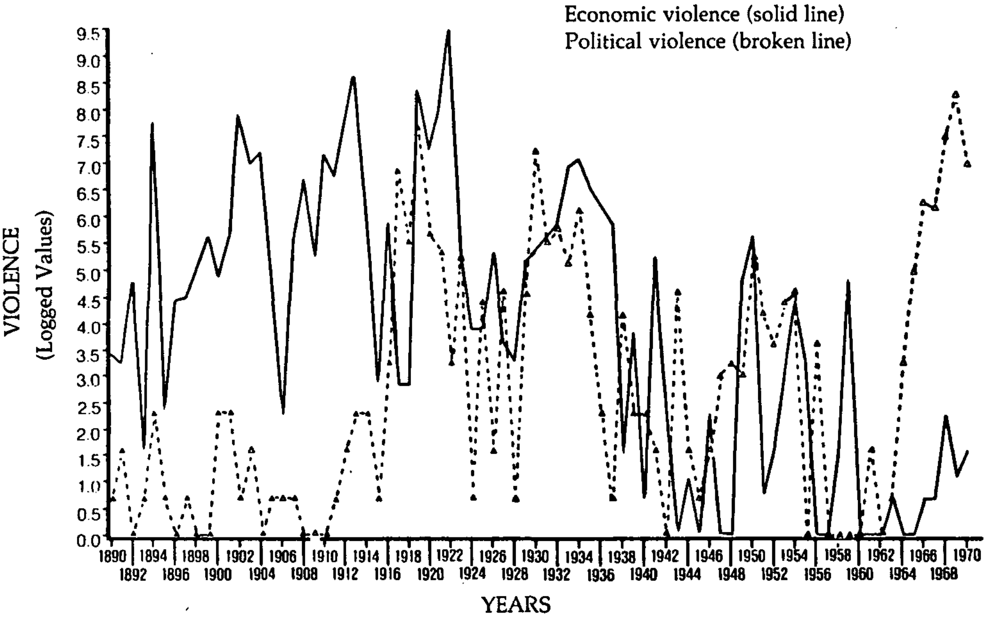
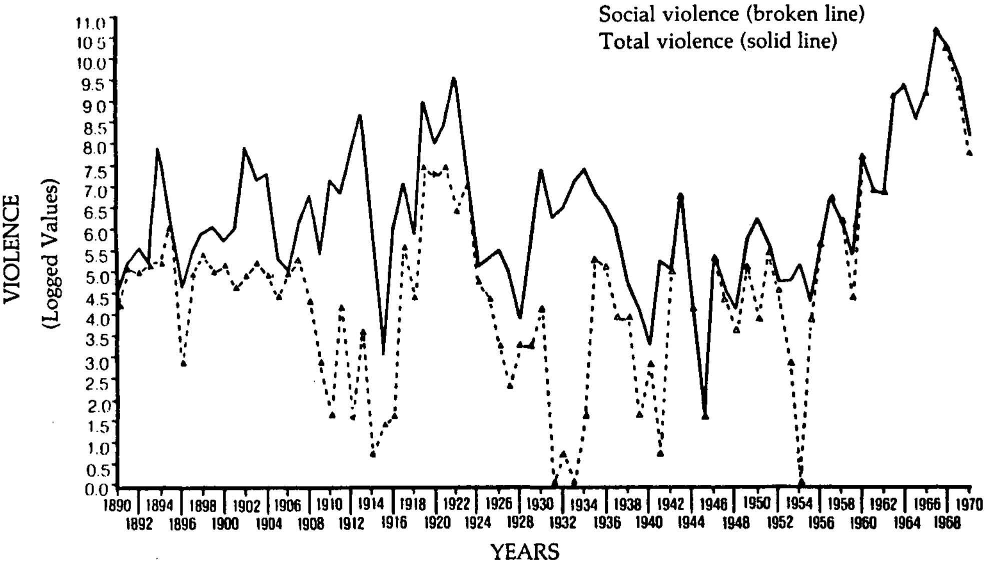


Figure 2. U.S. Social and Total Violence, 1890–1970



(events initiated by status quo groups), anti-system (events initiated by groups seeking to reform or challenge the status quo), and clash (events unidentified as either pro- or anti-system) activities. Because a large proportion of the data are identified as clashes, an extensive examination of each series is unnecessary. Nevertheless, the magnitude of pro-system violence will be used to discern whether repression (initiated by either the government or other dominant groups) is indeed higher in crises involving nonaccommodating political regimes than in crises involving conciliatory regimes.[6]

Statistical Approach

The development of Box-Tiao impact assessment models has made the utility of quasi-experimental designs a more attractive venture. They are designed to control the effects of autocorrelation and trend as the net effects of a theoretical change in the level of a time series are estimated. Moreover, they yield a direct measure of the impact of war by providing a numerical estimate of the differences between war and nonwar observations. Previous time-series studies (e.g., Stohl, 1975, 1976) have relied on indirect assessments by comparing prewar to postwar observations, or confounding the war years with either the prewar or postwar series. Therefore, Box-Tiao models will be employed to estimate the separate influences of war, postwar demobilization, and the Great Depression on domestic violence. The general Box-Tiao impact model can be summarized as

$$Y_t = f(I_t) + N_t. \qquad (1)$$

The intervention component, $f(I_t)$, is represented by one or two transfer function parameters, ω_0 and δ, which respectively estimate the initial impact of an event on $Y_t(\omega_0)$ and the rate of growth or decay in the level of the time series after the impact (δ).[7] The omega parameter (ω_0) is an estimate of the difference between pre- and post-intervention levels of Y_t; the delta parameter (δ), constrained between -1 and $+1$, reflects how quickly the post-intervention series reaches equilibrium. When δ approximates 1, the post-intervention series returns to equilibrium very slowly.[8] Conversely, when δ approximates zero, the post-intervention series returns to equilibrium very quickly. The noise component (N_t) encompasses the random and deterministic effects of trend and autocorrelation. It is modelled through an ARIMA approach, which estimates a linear filter(s) that transforms the observed time series (Y_t) into white noise.

After the initial step of identification and estimation of the noise model (ARIMA structure), the intervention component is selected based on whether the impact is abrupt or gradual and permanent or temporary.[9] The intervention parameter(s), in addition to the ARIMA parameter(s), are then estimated in a full model through iterative, nonlinear estimation procedures (Box and Jenkins, 1976; Box and Tiao, 1975).[10] Any nonstatistically significant parameters are removed before a final model is reestimated. Eventually, the goodness-of-fit of the model is judged by the degree of correspondence between the observed and predicted output series values, the residual mean square error, the statistical significance of the model parameters, and the "white noise," or random character of the residuals.

Specification of the Interventions

The impact of U.S. war involvements on domestic violence will be estimated for each violence time series in the form of three interventions: one encompassing the combined effects of the Spanish-American, Korean, and Vietnam wars, and a second and third reflecting the separate influences of World Wars I and

Table 1. The Impact of War on U.S. Domestic Violence, 1890–1970

	Intervention Component[a]				Noise Component			
	Interstate Wars	World War I		World War II	ARIMA	Type of Noise	Goodness-of-Fit[c]	
Violence Series	ω_0	ω_0	δ	ω_0	Parameters	Structure[b]	χ^2	RMSE
Economic	.19 (1.1)	-3.7* (2.6)		-.69 (.66)	$\theta_1 = .76^*$ (9.5)	(0,1,1)	16.1	2.9
Social	-.14 (1.1)	2.1* (2.4)	—	.29 (.56)	$\theta_1 = .49^*$ (4.4)	(0,1,1)	13.8	2.8
Political	.51 (1.5)	4.1* (2.7)	-1.1* (3.6)	-.09 (.14)	$\phi_1 = -.51^*$ (5.1)	(1,1,0)	9.4	3.1
Total	-.15 (.35)	-.07 (.87)	—	-.88 (1.2)	white noise	(0,1,0)	19.3	1.9

Note: All data have been logged to achieve variance stationarity; *t*-values are reported in parentheses; an asterisk indicates statistical significance at .05 alpha level.

[a] All intervention models are abrupt, temporary with exception of abrupt, or permanent models for economic violence during World War I.

[b] (0,1,1) denotes differenced, first-order moving average process;
(1,1,0) denotes differenced, first-order autoregressive process;
(0,1,0) denotes differenced process.

[c] Chi-square values indicate residuals are white noise with 11 degrees of freedom; RMSE represents residual mean square error.

II.[11] The influences of the interstate wars are combined, because of the expectation they will not have strong statistically significant associations with internal violence, either separately or jointly.[12] In those cases where the parameter estimates indicate otherwise, the separate influences of each war will be calculated; the same approach will be used to estimate the postwar effects. Each postwar demobilization period will be equal to the number of years involved in actual warfare.[13]

Meanwhile, a separate estimation of the Great Depression's influence will be made for each of the four time series. An examination of the annual expansion and contraction periods of U.S. gross domestic product from 1886 to 1980 shows the years 1930-1933 to be associated with one of the worst declines in the U.S. national economy (The Economist, 1982, p. 24). Hence, these years will be used for specifying the intervention effects of the domestic conflict series.

Data Analysis

The Impact of War on Domestic Violence

Table 1 summarizes the statistical outcomes of applying Box-Tiao impact assessment models to the four continuous series of domestic violence. The collective impacts of the interstate wars are expected to yield statistically insignificant impact parameters, although the two global wars will be associated with statistically significant estimates. Considering that some levels of violence occur in any political system, statistical significance is an important criteria for identifying historical deviations from the norm. Even though the short term consequences of wartime influences on internal violence are important, primary interest is whether these influences are responsible for unusual levels of severity in comparison to nonwar years. The results shown in Table 1 indicate that the only significant impacts of war are associated with World War I.

With the exception of economic-related violence, the direction of these impacts shows positive increases for the remaining forms of violence.

My initial strategy was to isolate the effects of global and interstate wars on internal violence. Mixed results, if any, between the global wars were to be accounted for through the intervening effects of government accommodation. Since the evidence does reflect contrasting conflict linkages between World Wars I and II, below I will interpret parameter estimates for economic, social, and political violence on the basis of the wartime policies pursued by national elites. As an aggregate measure, the total violence series will yield parameter estimates that either support the overall findings of the other violence series or show, as in Table 1, how aggregation can distort important underlying relationships. Any further analysis of the series is unnecessary.

Economic Violence. Ordinarily, the omega (ω_0) estimates in Table 1 express the immediate impact in terms of the numerical units of the dependent variable. Where the data have been logged, the numerical estimates are converted to percentages to reflect the change in the level of a series.[14] In light of the log transformations herein, the impacts of World Wars I and II are accompanied by a 98% and 50% decline, respectively, in economic-related violence. Although World War I is associated with the only statistically significant decline, the findings suggest that economic violence—a large proportion of which derives from labor-management disputes—did not increase at a time when national officials would have been most vulnerable. According to labor historians, the two world war experiences offered extraordinary opportunities for American labor leaders—who were normally locked out of the political process—to gain access to the inner circles of national decision making.

During the world wars, the Wilson and Roosevelt administrations made a concerted effort to enlist labor's cooperation to smooth and sustain the process of economic mobilization. Their policies led not only to major reforms on behalf of labor, but also to the sponsorship of labor participation in the national government.

Both Wilson and Roosevelt established a National War Labor Board (NWLB) involving the participation of prominent labor officials, for the purpose of formulating a comprehensive set of rules to guide labor-management relations during the war. Many of the new principles involved significant labor reforms. During World War I these included the recognition of workers' rights to join independent trade unions, the preservation of the union shop and union conditions where they already existed, an eight-hour workday, and equal pay for women (Dubofsky, 1975, p. 122). During World War II, they disposed of any remaining barriers to collective bargaining, and extended labor-management issues beyond wage topics to include fringe benefits, wage-rate inequalities, piece rate computation, and geographical differentials (Brody, 1980, pp. 112-16). Meanwhile, the A.F.L. (in World War I) and the C.I.O. (in World War II) agreed to no-strike policies, actively squashed strikes, and aided the government in purging disloyal, subversive elements—members of the International Workers of the World (IWW) and of the Socialist and Communist parties—from the labor movement.[15]

Although the war years were characterized by greater government flexibility regarding labor issues, they did not avoid a substantial rise in labor strikes. Between 1914 and 1916 an average of 2,195 work stoppages occurred, in comparison with 4,450 in 1917 and 3,353 in 1918 (U.S. Bureau of the Census, 1975, p. 179). Yet empirical findings indicate economic violence declined significantly during World War I. This phenomenon can be attributed partially to the efforts of federal mediation in labor-management disputes, the influence of A.F.L. officials in persuading workers to return to their jobs, and, in cases where these options failed, government repression of union leadership—for example, in the IWW-led strikes in copper, lumber, and food grains (Dubofsky, 1975, pp. 115-21).

The pattern is similar in World War II, though less dramatic. The average number of work stoppages in the four years preceding the war (1938-1941) increased from 3,045 to an average of 4,107 during the war years (1942-1945) (U.S. Bureau of the Census, 1975, pp. 179). Nevertheless, there is an empirical decline, albeit weak, in economic violence. Again, the harmonious labor-management relations fostered by the government's support of collective bargaining is probably a crucial factor. In the final analysis, the political contexts of World Wars I and II, in addition to the quantitative evidence, contribute support for the hypothesis that accommodating political regimes will contribute to weak or nonexistent conflict linkages.

Social Violence. The empirical evidence in Table 1 indicates that World War I is associated with a statistically significant (717%) increase in social violence; World War II is affiliated with a statistically weak (34%) increase. Bearing in mind that 95% of the social violence events for the 1890-1970 era involve racial violence, the role of national accommodation will be analyzed on the basis of the Wilson and Roosevelt administrations' wartime record on black civil rights issues.[16]

During World War I, the Wilson administration failed to undertake significant reforms on behalf of blacks. It ignored basic demands by blacks for the elimination of military desegregation, federal employment discrimination, and Jim Crow practices in the federally controlled railroad system (Scheiber and Scheiber, 1969, pp. 454-57). Prewar discriminatory practices by employers and white union officials were sustained during the war (with the approval of the federal government), and black workers had little or no opportunity to advance from unskilled to semiskilled occupations (Foner, 1982, pp. 133-34). An examination of the U.S. Supreme Court civil rights decisions from 1876 to 1955 reveals a significant decline both in the volume of cases and the number of pro-civil rights outcomes between 1916 and 1920 (McAdam, 1982, p. 85). Needless to say, national political elites were far from accommodating the political and social interests of blacks in American society.

The concomitant rise in social violence during this period is attributed in large part to the conflict between blacks and whites over urban issues of housing, transportation, recreational facilities, and the competition for jobs and wages. Despite pleas from black leaders, Wilson refused to take public positions denouncing the practice of lynchings and the failure of local authorities to protect black citizens (Scheiber and Scheiber, 1969, pp. 455-57).

The Roosevelt administration, on the other hand, undertook major reforms during World War II to accommodate the interests of blacks. In 1943, the War Labor Board outlawed wage differentials based on race; the U.S. Employment Service reversed existing policy by refusing to honor requests specifying the race of applicants; and the National Labor Relations Board stated it would refuse to certify unions excluding minority groups. By the end of the war, the federal government had increased the number of its black employees from 60,000 to 200,000, and more often in higher classifications (Polenberg, 1972, p. 116). Finally, the volume of U.S. Supreme Court decisions on civil rights cases—many of which were aimed at striking down Jim Crow practices—and the number of favorable outcomes increased from 1941 to 1945 (McAdam, 1982, p. 85). In short, the political accommodation during World War II is associated with a statistically weak conflict linkage; the lack of accommodation in World War I is linked with a statistically strong conflict linkage.

Political Violence. The empirical evidence indicates that World War I is associated with a strong (603%) increase in political violence, and that World War II has a weak negative (9% decline) association. In the case of World War I, there is a noticeable unstable effect on political violence. The Box-Tiao results in Table 1 show statistically significant omega (ω_0) and delta (δ) parameters. Ordinarily, when values equal or exceed 1.0, the delta parameter is dropped from the equation and then the omega parameter is estimated alone. However, this procedure results in a weaker and statistically insignificant omega estimate.[17]

An examination of Figure 1, which plots the political violence series from 1890 to 1970, reveals that the substantial increase in 1917 is followed by a subsequent decline in 1918. This contributes to the unusually large, negative δ, and to a statistically insignificant estimate of ω_0 without the presence of the δ parameter. Nevertheless, the magnitude of political violence in 1918 is considerably higher than any of the prewar observations from 1890 to 1916. Therefore, despite the appearance of misspecification, the earlier estimates are maintained as the appropriate findings, and more than likely reflect the problem of a short intervention series that has failed to establish equilibrium (McCleary and Hay, 1980, p. 159).

Much of the political violence between 1917 and 1918 is the result of actions taken by government agencies and other status quo groups against leftists, farmers, and pacifists protesting the war and selective service laws (Peterson and Fite, 1975; Stohl, 1976, pp. 91-95). Members of the Socialist Party and the International Workers of the World were prominent targets, since their organizations took strong public stands against the war. Their rallies, parades, and assemblies often precipitated attacks at local levels. The IWW was an especially popular object of repression by local and national authorities, who used its antiwar position as a justification for eliminating its disruptive labor union operations (Peterson and Fite, 1975, pp. 48-60).

The 9% decline in political violence during the World War II years is attributed to the impact of Pearl Harbor on public consensus, the absence of major violence in interning the Japanese-American population, and to the presence of the Soviet Union as a major American ally, which helped to unify leftist support (Brooks, 1979, pp. 314–15). The lack of opposition to the war, in contrast to the 1917-1918 years, makes it difficult to pin down the role of accommodation for the Roosevelt administration. Since national elites were never required to respond to any large constituency, it is not possible to link the position of government officials to the weak, negative relationship between World War II and political violence.

The only other significant finding in this section is the strong positive impact of the Vietnam War on political violence. Since the estimation of the collective influences of interstate wars yielded contradictory estimates, the intervention components were respecified to calculate the separate influences of the Spanish-American, Korean, and Vietnam wars on political violence. The results indicate that the Vietnam War is accompanied by a statistically significant (949%) change in the level of the series. This finding supports earlier evidence of a positive association between the escalation of U.S. troop levels and the rate of increase in the number of antiwar protests from 1964 to 1968 (Tanter, 1969, p. 557). However, the

Table 2. The Impact of Postwar Demobilization on U.S. Domestic Violence, 1890–1970

	Intervention Component[a]				Noise Component			
	Interstate Wars	World War I		World War II	ARIMA	Type of Noise	Goodness-of-Fit[c]	
Violence Series	ω_0	ω_0	δ	ω_0	Parameters	Structure[b]	χ^2	RMSE
Economic	-.37	3.9*	-1.0*	.58	θ_1 = .63*	(0,1,1)	17.4	4.1
	(.73)	(2.3)	(3.6)	(1.1)	(6.2)			
Social	.78	3.6*	-.97*	.31	θ_1 = .46*	(0,1,1)	12.3	2.8
	(1.5)	(2.1)	(3.9)	(.56)	(4.2)			
Political	-.76	1.4*	—	.69	ϕ_1 = -.45*	(1,1,0)	4.5	3.0
	(1.3)	(2.0)		(1.1)	(4.1)			
Total	.13	3.2*	-1.0*	.22	white	(0,1,0)	19.8	2.0
	(.20)	(2.3)	(2.5)	(.30)	noise			

Note: All data have been logged to achieve variance stationarity; *t*-values are reported in parentheses; an asterisk indicates statistical significance at .05 alpha level.

[a] All intervention models are abrupt, temporary with exception of abrupt, or permanent models for economic violence during World War I.

[b] (0,1,1) denotes differenced, first-order moving average process;
(1,1,0) denotes differenced, first-order autoregressive process;
(0,1,0) denotes differenced process.

[c] Chi-square values indicate residuals are white noise with 11 degrees of freedom; RMSE represents residual mean square error.

violence data fail to cover the total period of American war involvement, making it premature to analyze the statistical results any further.

The Postwar Impact of Demobilization on Domestic Violence

Table 2 presents the results of estimating the Box-Tiao impact assessment models for the collective and separate influences of the interstate wars and World Wars I and II on domestic violence. A preliminary overall evaluation indicates that the post-World War I period (1919-1920) is associated with statistically significant increases for all forms of domestic violence; the individual and aggregate postwar periods of World War II and the interstate (i.e., Spanish-American and Korean) wars show positive but statistically weak increases. A full interpretation of these results is reserved for the following sections.

Economic Violence. The parameter estimates in Table 2 show that the post-World War I period (1919-1920) is accompanied by a statistically significant (603%) increase in the level of economic violence, whereas the post-World War II era (1946-1949) is linked with a statistically weak (79%) increase. The large negative value of the post-World War I δ reflects the similar unstable patterns of political violence during the war. An examination of Figure 1, which plots the logged values of the series from 1890 to 1970, reveals that the postwar impact has its strongest influence in the initial postwar year (1919). The level of violence is not sustained in 1920, but is still considerably higher than the 1917-1918 levels. The short intervention series coupled with the decline in 1920 yields another statistically insignificant omega estimate without the δ parameter.[18] Therefore, the initial parameter estimates are retained.

In comparison with the major reforms on behalf of labor during World War I, the postwar period was characterized by a return to national conservatism. By mid-1919, labor unions lost many of the reforms gained during the war. Both the U.S. government and management no longer endorsed and maintained agreements on wage levels, the length of the workday, and codes on industrial safety. Most significantly, collective bargaining was no longer embraced by governmental officials (Dubofsky, 1975, pp. 122-31; Hawley, 1979, pp. 45-52).

Therefore, when labor-management disputes occurred between 1919 and 1920, the resulting labor strikes generated an increase in the level of economic violence. It is difficult to overlook the decline in economic violence accompanying the large number of work stoppages during World War I ($\bar{x}$ = 3902), when a relatively equal number of work stoppages in 1919-1920 ($\bar{x}$ = 3521) is associated with a statistically significant increase. The absence of a national government promoting and enforcing policies of collective bargaining removed an important avenue for diffusing potential violence.

The post-World War II years (1946-1949) are also characterized by a large number of work stoppages ($\bar{x}$ = 3926); and though there is an increase in economic violence, the change is statistically weak from a historical perspective.[19] Like the 1919-1920 years, the post-World War II era also saw a return to public and Congressional conservatism regarding the economic position of labor unions, federal spending, and American foreign policy abroad. Nonetheless, national administrative elites went against these trends in their efforts to regenerate the American economy and supervise the European Recovery Program.

Truman made every effort to coopt major American labor leaders in an attempt to obtain internal economic stability and enlist their aid in mobilizing domestic support for the Marshall Plan (Freeland, 1972; Godson, 1976; McClure, 1969). Stimulated by the reasoning that future economic growth would produce abundance and material gain for all classes without a redistribution of economic power, C.I.O. and A.F.L. labor leaders endorsed the administration's policies (Hamby, 1974, p. 66; Milton, 1982, pp. 154-55). They compromised on labor-management battles designed to expand their role in management decisions, and accepted the proposition of pegging wage increases to cost-of-living and economic productivity levels (Brody, 1980, pp. 173-213). As representatives of the U.S. government, they also participated in selling the merits of the Marshall Plan to the West European trade unions. Eventually, they would succeed in splitting the European labor movement between Communist and non-Communist elements, and contribute to the development of European political institutions on a more centrist political path (Maier, 1978, pp. 40-46). According to labor historians, these actions would lead to labor's loss of political independence, and insure labor peace over the course of the next 25 years (Brody, 1980, pp. 229-55; Milton, 1982, pp. 154-67). This position is supported by the quantitative evidence in Figure 1, which shows a decrease in the overall level of economic violence from 1946 to 1970.

Social Violence. The Box-Tiao estimates in Table 2 indicate that the post-World War I era is accompanied by a statistically significant (522%) increase in the level of social violence; the post-World War II era is linked with a statistically weak (36%) increase. The political climate for blacks in 1919 and 1920 was far from favorable. Blacks in America went into the war with high morale generated by the belief that a new democratic order would be achieved on the home front. Instead, black soldiers met with discrimination and segregation, and when they came home, they faced Jim Crow and unemployment. They also found the Klu Klux Klan revitalized, a denial of suffrage and residential segregation as before, and a gathering of racial tensions that led to the "Red Summer" of 1919. In one year, 77 blacks were lynched and 26 American cities suffered race riots (Scheiber and Scheiber, 1969, pp. 455-58).

Despite the post-World War I years, America's rise to world leadership status after World War II had unintended consequences for internal policies of racial discrimination. Locked in what was perceived to be an ideological struggle against Communism, national leaders viewed racial discrimination as a major handicap in the Cold War competition for influence over nonwhite Third World nations (Berman, 1974, p. 186; Bernstein, 1974, p. 58; Myrdal, 1970, p. 35). Furthermore, the strategic location of black voting blocs in northern states with large electoral votes made blacks an important element in Truman's bid for the presidency in 1948 (Berman, 1974, p. 186). These international and domestic realities prompted Truman to take increasingly progressive actions that led to (1) the desegregation of the armed forces, (2) the appointment of a Civil Rights Committee, and (3) the establishment of a Fair Employment Board within the Civil Service Commission. In February 1948, Truman introduced the first comprehensive civil rights program to the Congress since the post-Civil War years, and three years later he closed out his term by establishing a Committee on Government Compliance, aimed at preventing discrimination in employment by private firms holding government contracts (McAdam, 1982, pp. 83-84).

Table 2 also indicates that the influence of the interstate wars on social violence is associated with a 118% increase in level. Due to the unusually large value, the separate postwar effects for the Spanish-American and Korean wars were estimated. The only statistically significant result is the 232% increase that occurs with a one-year lag of the post-Korean War (1955-1957) impact. The substantial rise in social violence follows the 1954 Supreme Court decision in *Brown v. Topeka Board of Education* on the issue of public school segregation. Southern state officials enacted a flood of prosegregation legislation in reaction. When the federal government failed to enforce the Supreme Court's ruling, black political resistance gained momentum throughout the South, and eventually resulted in an escalation of violence (McAdam, 1982, pp. 144; Piven and Cloward, 1977, pp. 207-8).

Political Violence. According to the Box-Tiao estimates in Table 2, the post-World War I years are charged with a statistically significant (290%) increase in political violence. The post-World War II years, on the other hand, are associated with a statistically weak (99%) increase.

The substantial levels of post-World War I political violence are due, in large part, to the sustained concern of national administrative elites with repressing groups and individuals espousing communist, socialist or anarchist views. Public distaste for radical groups such as the Socialist party and the IWW intensified with exaggerated threats of bolshevism spreading from Europe to the U.S. Public demonstrations and assemblies by radical organizations continued to be attacked by local populations with the tacit approval of local and national officials (Peterson and Fite, 1975, pp. 285-306).

The role of national accommodation during the post-World War II years is difficult to discern closely, because, as with the war years, there is very little organized political activity protesting or making public demands for or against government policies. Although the political climate became more antagonistic toward Communists (or alleged Communists), the reactionary elements of anticommunism did not dominate public attitudes until after 1947, with the inten-

Table 3. The Impact of the Great Depression on U.S. Domestic Violence, 1890–1970

	Intervention Parameters[a]				Goodness-of-Fit[d]	
Violence Series	ω_0	δ	ARIMA Parameters[b]	Type of Noise Structure[c]	χ^2	RMSE
Economic	1.2*	.79*	$\theta_1 = .75$*	(0,1,1)	12.8	3.9
	(2.0)	(4.7)	(9.3)			
Social	-3.7*		$\theta_1 = .39$*	(0,1,1)	10.2	2.5
	(3.5)		(3.5)			
Political	.33		$\phi_1 = -.46$*	(1,1,0)	7.6	3.1
	(.51)		(4.9)			
Total	.35		white noise	(0,1,0)	19.3	1.9
	(.49)					

Note: All data have been logged to achieve variance stationarity; *t*-values are reported in parentheses; an asterisk indicates statistical significance at .05 alpha level.

[a] The intervention model is gradual, permanent for economic violence; abrupt, permanent for social violence; and abrupt, temporary for political and total violence.

[b] The impact is lagged one year for social violence.

[c] (0,1,1) denotes differenced, first-order moving average process;
(1,1,0) denotes differenced, first-order autoregressive process;
(0,1,0) denotes differenced process.

[d] Chi-square values indicate residuals are white noise with 12 degrees of freedom. RMSE represents residual mean square error.

sification of the Cold War. Truman's national security concerns vis-a-vis the Soviet Union shaped domestic priorities toward a preoccupation with radical subversion, both abroad and at home. The federal employee loyalty program and the trials of American Communists during these years fostered rigid conservatism among national elites and contributed to the rise of Joseph McCarthy in the 1950's (Theoharis, 1970, pp. 197-232). Nevertheless, there is no sustained level of organized political activity similar to the Socialists and Wobblies (IWW) during and after World War I. Consequently, national accommodation cannot be linked in any obvious way with the empirical findings.

The Impact of the Great Depression on Domestic Violence

Economic Violence. Table 3 reveals that the depression years (1930-1933) are accompanied by a statistically significant increase in the level of economic violence. The first year (1930) is linked with an initial change (232%) in violence that continues to escalate through 1933.

Much of the economic violence in the early years (1930-1932) of the depression involved the resistance of industrial workers to substantial wage cuts and substandard working conditions. Meanwhile, the Hoover administration opposed all federal proposals dealing with the problems of unemployment (e.g., public works programs, unemployment insurance, federal relief policies) and labor reform. The only labor legislation Hoover signed, with obvious reluctance, was the 1932 Norris-LaGuardia Act designed to prevent federal officials from issuing injunctions prohibiting labor strikes, union membership, picketing, and other actions restricting labor activity. The source of the bill came from Congress, and Hoover and his administration worked secretly, though unsuccessfully, to block its passage (Bernstein, 1960, pp. 397-415, 505-8).

However, the situation changed in 1933, when Roosevelt enacted Section 7a of the National Industrial Recovery Act (NIRA), endorsing labor's right to organize and bargain collectively with management over issues of wages, working hours, and industrial safety codes. The unanticipated consequence was a significant rise in economic violence, owing primarily to employer resistance to collective bargaining. In the final analysis, the rise in economic violence from 1930 to 1932 is associated with a nonaccommodating political administration, and the sustained escalation of violence in 1933 is linked with a reform-oriented administration. Although the level of the 1933 violence does not support theoretical expectations; it can nevertheless be attributed to the conservative positions of business and corporate elites who, because they believed the NIRA to be unconstitutional, decided not to comply (Bernstein, 1970, pp. 172-216; Milton, 1982, p. 31). Between 1934 and 1941, 51% of the work stoppages were over the issue of union recognition; by the end of World War II (1942-1945), the average was down to 21%.[20] There was an obvious time lag between the federal government's endorsement and its acceptance by nongovernmental elites, who did not fully embrace the concept until the procedures of collective bargaining were formalized via the National War Labor Board during World War II (Brody, 1980, pp. 173-214). Therefore, the rise of economic violence in 1933 can still be linked with a nonaccommodating political environment, despite the position of the federal government.

Social Violence. The Box-Tiao estimates in Table 3 indicate a statistically significant (98%) decline in social violence from 1931 to 1933. This is an unexpected result, considering the lack of action by the Hoover administration to alleviate the plight of blacks during the depression. Although blacks received limited political and economic gains from the Roosevelt government, the absence of violence in 1933 is not sufficiently different from the state of affairs in 1930-1932 to attribute it to national accommodation.[21]

More likely, the decline is attributable to the participation of blacks with whites in Communist-organized hunger marches, public demonstrations, and labor strikes—many of which ended in violence (Foner, 1982, pp. 188-203; Piven and Cloward, 1977, pp. 48-91). Therefore, much of the violence involving blacks during these years took place within the contexts of labor-management disputes (reflected in economic violence) and political opposition between governmental officials and the unemployed public (reflected in political violence).

Political Violence. The statistical estimates in Table 3 reflect that political violence shows a statistically weak (39%) increase from 1930 to 1933. A substantial amount of this violence is the result of the mobilization of large numbers of people, primarily unemployed, who demonstrated and clashed with state and national authorities, protesting the lack of jobs and public assistance. Largely organized by the Communist party and other radicals, the public engaged in massive hunger marches, resisted tenant evictions, and stormed government offices (Piven and Cloward, 1977, pp. 49-60). Although there was very little accommodation by the Hoover administration during the first three years of the depression (1930-1932), the level of political violence does not reach a critical threshold.

Linkages Between Repression and National Accommodation

The last hypothesis in this study associates nonaccommodating political

Table 4. A Comparison of the Average Magnitude of Pro-System Initiated Violence during Crisis and Noncrisis Years

Noncrisis Years	Pro-System Violence	Crisis Years	Pro-System Violence
1900–1909	6	1917–1918[a]	183
1910–1916	29	1919–1920[a]	1215
1921–1929	54	1930–1932[a]	134
1934–1939	143	1933[b]	168
1940–1941	23	1942–1945[b]	89
1950–1959	35	1946–1949[b]	58

[a] During this period there was no national accommodation toward major economic, social, and political groups (with the exception of reforms on behalf of labor during 1917–1918).

[b] National accommodation toward major economic and social groups occurred during this period.

administrations with higher levels of repression than reform-oriented regimes. Nonaccommodating governments are more likely to use coercion in order to suppress domestic challenges, thus escalating the level of internal violence. Reform governments, on the other hand, are less likely to rely on repressive measures, thereby reducing potential levels of violence. A test of the hypothesis is based on a comparison between the levels of violence initiated by pro-system groups during World Wars I and II and their postwar periods, and during the Hoover (1930-1932) and Roosevelt (1933) administrations during the Great Depression. Unfortunately, the outcome will not be conclusive, inasmuch as the pro-system violence includes the actions of both governmental elites and dominant political elites. However, if nongoverning elites take cues from the behavior of national officials regarding appropriate and inappropriate responses to new contenders, there is apt to be a correlation among their actions, despite the obvious exceptions (e.g., the response of corporate elites to Section 7a of the NIRA in 1933, and the reaction of southern officials to the Supreme Court decision on public school segregation in 1954). Therefore, pro-system violence is still a useful measure for comparing levels of repression among U.S. administrations.

Table 4 tabulates the average magnitude of pro-system violence for each decade from 1900 to 1959 (including the crisis years) in comparison with the average magnitude of pro-system violence for each crisis period. The evidence indicates two things. First, the overall levels of repression are higher during crisis years than in noncrisis periods, regardless of accommodation. This is not unexpected, in view of the greater insecurity among governing and nongoverning elites during critical periods.

Second, a rank order of the repression during the crisis periods shows that two of the three nonaccommodating regimes (1917-1918 and 1919-1920) are associated with the highest levels of repression; two of the three conciliatory regimes (1942-1945 and 1946-1949) are linked with the least amount of repression. The Hoover and the Roosevelt administrations present the anomalies. The data suggest that the Roosevelt administration's first year, though more accommodating than Hoover's, was associated with more repression. However, this result is more the consequence of the activity of business elites, who refused to recognize collective bargaining, than of governmental action.

Table 5. The Association of U.S. National Accommodation and the Linkages between Crisis and Internal Violence

	Accommodation		Nonaccommodation	
Type of Contenders	Crisis	Conflict Linkage[a]	Crisis	Conflict Linkage
Economic	World War I World War II Post-World War II	Strong, negative Weak, negative Weak, positive	Post-World War I Depression[b]	Strong, positive Strong, positive
Social	World War II Post-World War II	Weak, positive Weak, positive	World War I Post-World War I Depression[c]	Strong, positive Strong, positive Strong, negative
Political	World War II Post-World War II	no test no test	World War I Post-World War I Depression	Strong, positive Strong, positive Weak, positive

Note: U.S. administrations that implement major reforms on behalf of nondominant economic, social, and political groups are labelled as accommodating; otherwise, administrations are considered nonaccommodating.

[a] Based on Box-Tiao parameter estimates indicating statistical significance (strong/weak) and the directional impact of war, postwar demobilization, and the Great Depression on economic, social, and political forms of violence.

[b] Nonaccommodation during 1933 reflects the resistance of dominant corporate and business elites to Roosevelt's policy on union recognition.

[c] Nonaccommodation during 1933 reflects the resistance of local elites to Roosevelt's policies of equal distribution of public welfare aid for blacks and whites.

The real question is why the level of repression is lower than one would typically surmise for the Hoover years. Perhaps public sympathy for the plight of the unemployed and homeless made the use of repression a less palatable option for everyone. If that is the case, then the public's approval of the claims advanced by a particular segment of the population can go a long way toward influencing the acceptable parameters of repression.

Conclusion

This study shows that an overemphasis on finding positive statistical associations between three types of crisis (war, postwar demobilization, and depression) and internal violence can overlook substantively important evidence that has wider theoretical implications. Particularly, it aims at demonstrating how the relationship between crisis and internal violence is conditioned by two potential factors: the severity of the crisis and the presence or absence of governmental accommodation. Table 5 shows that the isolation of these variables during the critical periods of World Wars I and II, their postwar eras, and the 1930's depression reveals 10 out of 13 cases where crises are associated with increases in American domestic violence. However, the crucial test involving the statistical significance of Box-Tiao parameter estimates indicates that 6 of these 10 cases depict historical periods of unusual levels of domestic violence, all of which are linked with nonaccommodating political environments. Conversely, 5 out of the 6 cases indicating a decline or weak positive increase in domestic violence are associated with accommodating U.S. political administrations. The overall evidence supports the hypotheses that link nonaccommodating and reform-oriented regimes with statistically significant and insignificant conflict linkages.

Although the levels of repression tend to be higher during crises than in noncrisis periods, nonaccommodating U.S. political administrations are associated more frequently with higher levels of repression than are conciliatory ones. The theoretical implications suggest that conservative political regimes are likely to generate higher amounts of repression during crises, thus escalating the magnitude of internal violence. When governmental elites recognize the legitimacy of the claims advanced by domestic groups (in at least a symbolic fashion), they are less prone to use repression, thereby breaking the cycle of intensive violence. When elites are willing to negotiate the claims of domestic contenders, conflict is likely to be preempted or substantially reduced, as demonstrated by the decline in economic violence with the presence of government-supervised collective bargaining in labor-management disputes during World Wars I and II.

Nevertheless, the reform policies of accommodating elites can also have the unintended consequence of stimulating more domestic violence. Two prominent illustrations are the 1933 National Industrial Recovery Act recognizing union representation in labor-management disputes and the 1954 Supreme Court decision that failed to endorse segregation in the public school system. Both events catalyzed the mobilization of labor and blacks against resisting forces. The subsequent attempts by labor and blacks to consolidate their new political gains brought renewed violence through clashes with nongovernmental elites and state and local officials who did not support national policies or views.

Consequently, decisions to support political reforms—no matter how limited they may be—can be very risky. If there is concern with maintaining internal stability during critical periods of management, national decision makers are faced with making crucial policies that may open up a Pandora's box of unexpected outcomes. Their tendency to rely on status quo policies and repression becomes more understandable. Therefore, the context that ultimately supports political change becomes an important issue. Under what conditions are crises likely to support governing elites who are willing to sponsor political reforms? As the evidence indicates, the presence of a serious crisis alone does not automatically ensure political concessions, regardless of the magnitude of domestic violence.

Crises that coincide with fundamental transformations in domestic political environments are more likely to facilitate important long-lasting concessions. For instance, there is the political realignment in the 1930's that fostered the Democratic Party's capture of electoral support from the lower socioeconomic strata of the voting population. Although this event by itself was not directly responsible for New Deal social welfare innovations (like the Social Security and Wagner Acts), it did force political leaders to overcome both administrative weaknesses in the national government and a hostile seniority system in Congress in order to act on welfare issues through the federal government rather than through regular party channels (Skocpol, 1980, pp. 186-201). While the concessions won by the public were mediated by the presence of both an accommodating government and internal violence, they are less likely to have been sponsored without the political realignment (Piven and Cloward, 1977, p. 31). Another example is the culmination of the long-term growth in the northern black electorate that coincides with major reforms on behalf of blacks during and after World War II (Dalfiume, 1974, pp. 181-84; McAdam, 1982, pp. 81-86).

So the lesson is that governments become sensitized to the issues put forth by challengers, but are only motivated to support them when the political environment makes it advantageous to do so. In short, concessions come when governmental officials receive something in return, whether it is labor peace, elected office, or support for international policies. Therefore, Block's (1977) and Skocpol's (1980) thesis that crises open up political space for new reforms appears to have some validity, but only if their timing intersects with other fundamental changes in domestic social and political arrangements. At least that appears to be the case in the American experience.

The most significant aspect of this study has been the theoretical benefits derived from linking the behavior of governmental officials with the empirical relationships between crises and internal violence. It reconfirms the necessity of thinking about domestic violence in terms of a process of actions and reactions among multiple actors (governmental and nongovernmental), and about how the process is affected by the appearance of crises like war, postwar demobilization, and depression. Finally, the extent to which the evidence herein is a reflection of other, non-American, experiences depends on future investigations that also see the advantages of using "processual" perspectives.

Notes

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1. Some studies that emphasize these themes are Almond, Flanigan, and Mundt (1973), Binder, Coleman, LaPalombara, Pye, Verba and Weiner (1971), Grew (1978), Skocpol (1979), Skowronek (1982), and Tilly (1975).

2. Although Stein (1978) investigates the relationship between war and domestic violence, the issue is subsumed within the broader framework of the effects of war on government centralization, industrial productivity, unemployment, and taxation. Violence is a minor consideration, and is used as an indicator along with crime rates and strikes to form a composite variable of social disunity. Consequently, his analysis is not included in the subsequent review.

3. Specific criticisms about the absence of intervening factors have been made by Skolnick (1974), Mack (1975), Stein (1976), and Bar-Siman-Tov (1983).

4. See Modelski (1974) and Thompson (1983) for conceptual distinctions between interstate and global wars.

5. These findings are not restricted to American international war involvements. The Civil War (1861-1865) is also associated with permanent effects similar to global wars.

6. Starting in 1935, Stohl (1976) subdivides prosystem violence into government-initiated and dominant group-initiated activity. In order to preserve the continuity from 1890 to 1970, the distinctions are not recognized in this analysis.

7. The intervention (I_t) is represented as a binary variable that ranges between zero (denoting the absence of the event) and one (denoting the presence of the event).

8. McCleary and Hay (1980) suggest that when the δ is equal to or exceeds 1.0, the level of the series changes by the quantity ω_0 in each post-intervention moment. In other words, before the intervention, the series is trendless; after the intervention, the series follows a trend with the parameter ω_0 interpreted as the slope. Such a radical change (from a state of equilibrium to a state of growth) is more than likely due to a post-intervention series that is too short to capture the equilibrium state of the process.

9. See Leng (1984), McCleary and Hay (1980), and McDowell, McCleary, Meidinger, and Hay (1980) for more details and illustrations.

10. The Pack (1977) computer program is used to estimate the final time series models.

11. The intervention components are coded to reflect the war years for 1898 (Spanish-American War), 1917-1918 (World War I), 1941-1945 (World War II), 1951-1953 (Korean War), and 1965-1970 (Vietnam War). Since America's entry into World War II began in December, 1941, the effects on domestic violence are not expected to have full realization until 1942.

12. Moreover, there are mathematical constraints that make it impossible to estimate five separate

intervention components with a time series of only 80 observations.

13. Therefore, the intervention components are coded to reflect the postwar years of 1899, 1919-1920, 1946-1949, and 1954-1956. Although there are no strict theoretical guidelines for setting the parameters of postwar periods, this procedure is considered the most conservative approach.

14. McCleary and Hay (1980, pp. 171-85) advance the following formula for translating log metric omega (ω_0) parameters into percentage of change intervention estimates:

$$\text{Percent Change} = (\epsilon^{(\omega_0)} - 1)100,$$

where $\epsilon^{(\omega_0)}$ is the ratio of post-intervention to pre-intervention equilibrium. However, in models with significant delta (δ) parameters, the incremental post-intervention movement of the process requires the analyst to exponentiate the asymptotic change ($\omega_0/1 - \delta$), in the process level.

15. For more details see Grubbs (1968), Larson (1975), and Milton (1982).

16. The social violence data exclude lynchings.

17. Attempts at estimating a higher order compound model that incorporates both the temporary changes in 1917 and 1918 also yield statistically insignificant omega estimates.

18. Estimation of a higher order compound model yields statistically insignificant omega estimates for the temporary changes in 1919 and 1920.

19. The work stoppage statistics for the post-World War I and post-World War II eras are based on figures obtained from the U.S. Bureau of the Census (1975, p. 179).

20. The figures are obtained from the U.S. Bureau of the Census (1975, p. 179).

21. Roosevelt's first administrative year, 1933, is characterized by largely symbolic gestures at this point, when New Deal leaders and agencies (e.g., the Federal Employment Relief Administration, Work Projects Administration, National Youth Administration, and Civilian Conservation Corps) made strong efforts to insure fair treatment of blacks. However, most of these and other programs faced major obstacles from local laws and customs, and federal officials were forced to capitulate on practical grounds (Fishel and Quarles, 1970).

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Erratum

Erratum in Robert Axelrod, "Presidential Election Coalitions in 1984" (March 1986, pp. 281–84):

Table 2, column 2, p. 283. The percentage contribution of Whites to the Republican coalition in 1980 was 98%, not 90%.

Forthcoming in December

The following articles and research notes have been scheduled tentatively for publication in the December 1986 issue:

Robert Axelrod. "An Evolutionary Approach to Norms"

Jonathan Bendor and Terry M. Moe. "Agenda Control, Committee Capture, and the Dynamics of Institutional Politics"

Bruce Bueno de Mesquita and David Lalman. "Reason and War"

Gregory A. Caldeira. "Neither the Purse nor the Sword: Dynamics of Public Confidence in the Supreme Court"

Robyn M. Dawes, John M. Orbell, Randy T. Simmons, and Alphonse J. C. van de Kragt. "Organizing Groups for Collective Action"

Michael Doyle. "Liberalism and World Politics"

James D. Morrow. "A Spatial Theory of International Conflict"

John R. Schloz and Feng Heng Wei. "Regulatory Enforcement in a Federalist System"

Joel Schwartz. "Freud and Freedom of Speech"

CONTROVERSY

DEFENDING THE WELFARE STATE

An ethical defense of the welfare state may, as Robert E. Goodin has argued, adumbrate the notion that responsibility to provide for "needy strangers" flows from the same source as family responsibility. But this view of the moral responsibility for protecting vulnerable people is open to challenge. In this Controversy, Jeffrey Obler takes issue on this point, and Goodin replies.

Goodin (1985c) seeks to defend the welfare state by showing that just as we have responsibilities for the welfare of our families, we also have responsibilities for the welfare of poor strangers. To make his case, he uses the method of "reflective equilibrium," whereby he posits the general moral principle that accounts for a widely held moral intuition, and then applies it to an analogous situation for which there is no definite sense of what is right or wrong. He argues that we firmly believe we do nave responsibilities for the welfare of our families (accepted moral intuition), because they are vulnerable to us (general moral principle). Our relations to poor strangers (for which there is no set moral intuition) are similar, in that poor strangers are also vulnerable to us. Given this essential similarity, he continues, we must also have responsibilities for poor strangers, which can be best discharged through state welfare.

While this defense is provocative and imaginative, it is not convincing, for, contrary to Goodin's claim, relations within families and among strangers are not analogous. There are moral reasons why we do not treat strangers the same way we treat out families, even when both are vulnerable. Our relations to family members are morally distinctive, because we often voluntarily assume the obligation to care for them, and we often feel affection and love for them that are kindled by a biological bond and nurtured by sustained reciprocal caring. Vulnerability alone cannot account for our familial duties. It is not a sufficient condition. Nor is it a necessary condition; we do not abdicate our responsibilities to family members who are not vulnerable. The analogy, therefore, is not valid, and without this analogy, Goodin's defense of the welfare state crumbles.

Vulnerability Is Not a Sufficient Condition for Family Responsibilities

Children

Reflecting on children's responsibilities for parents and siblings, Goodin dismisses the argument that we have familial duties because we voluntarily assume them. He reminds us that "you do not ask to be born; you do not choose your parents or siblings" (Goodin, 1985c, p. 777). Given the apparent irrelevance of self-assumed obligation, he concludes that vulnerability must be the sole source of our responsibilities. However, another conclusion is more plausible: in the absence of self-assumed obligations, children's duties are far weaker than those of parents and spouses. Our moral intuitions deny that siblings have a duty to provide each other financial support, and under some circumstances, children's duty to support their parents is doubtful, though there is little doubt that parents must care for their young offspring and spouses must care for one another. Public policy mirrors this difference. When the federal government determines eligibility for Supplementary Security Income, it takes into account the resources of parents and spouses but ignores those of children and siblings (*Social Security Handbook*, 1984, p. 308).

This is not to deny that children have a greater responsibility for parents and siblings than for strangers. This may be explained, however, by feelings of friendship and gratitude that have been fostered over years of reciprocal giving. Goodin rejects reciprocity as the basis for responsibility, pointing out that what children owe their parents does not depend on what they have received from them. Children's duties, he says, do not vary according to how long they remained at home. Nor can they repay debts the way they pay off a mortgage. "A favor and a return-favor do not cancel each other out and dissolve the relationship; instead they strengthen it" (Goodin, 1985c, p. 778).

Precisely! And if favors are never returned, the relationship atrophies. English (1979) says that children have no responsibilities for estranged parents. She says that parents who have nothing to do with their son because he married outside their religion cannot expect his support if they should need help. Embedded in ongoing relationships, she says, children's obligations cannot be taken for granted. For Blustein (1982), children owe their parents a debt of gratitude for having carried out even minimal parental duties. Parents sacrifice this gratitude when they neglect their duties; nothing is owed them for simply having borne a child. Contrary to Goodin's claim, then, there is a connection between what parents have done for their children and what children owe their parents.

Parents

Many kinds of people can and do care for small children. Yet our moral intuitions tell us that natural parents have a special responsibility for the children they bear. The basis of this is self-assumed obligation. In a society where natural parents are expected to rear their offspring, people who decide to have children, or decide to accept the risk of having children, voluntarily assume the responsibility to care for them until they achieve independence. For Blustein (1982, pp. 147-48), the guiding moral principle is that "people are responsible for the forseeable consequences of their voluntary acts." The contractual quality of the bond is underscored when biological parents sign an agreement to relinquish their rights over and duties toward their child, and adoptive parents sign an agreement accepting responsibility for the child's care.

While he does not directly address this issue, Goodin, given his position on responsibilities, would surely reject the argument that biological parents have special obligations. This argument, he would say, mistakenly confuses "causal" and "task" responsibilities. He says that people who create a difficulty are not necessarily required to resolve it (Goodin, 1985c, p. 780). This task should fall on those best equipped to deal with the difficulty, whether or not they caused it. He presumably would argue that while indigent parents bring vulnerable children into this world, the responsibility for providing for the children's material support should be assigned, at least in part, to more prosperous taxpayers. In fact, Aid to Families with Dependent Children does shift at least part of the caring responsibility from poor biological parents to more affluent strangers.

To accept that the state has a duty to care for defenseless children, however, is not to concede that the source for this duty is the same for the state as it is for parents. Adoptive and natural parents differ from the state in that they voluntarily agree to rear children. As O'Neill (1979, pp. 30-31) puts it, "Only if the state controls and mandates procreation—as in Plato's *Republic*—could one hold that the state acquires the same sort of obligations as persons do by deciding to procreate, so undertaking to rear." The fact that the state cares for children who have been abandoned by their parents does not exonerate the parents from moral guilt. This guilt is meted out because parents have a special responsibility, rooted in their choice to bear children. If the state has a duty, its source is the children's vulnerability, but vulnerability alone cannot account for the parents' responsibility. Goodin's analogy simply does not work.

Spouses

Goodin's case seems particularly weak with regard to responsibilities for spouses, for after all, people do accept the obligation to care for each other when they voluntarily choose to marry. Yet Goodin insists that the marriage contract falls short of an ideal legal contract, inasmuch as its provisions are set by the state and not by the marriage partners. Even if this is so, self-assumed obligation is still the basis of responsibilities between spouses. A person cannot deny the duties assigned to him by an agreement he has voluntarily signed, on the grounds that he did not write it. Having composed a contract is not a legal or a moral condition for honoring it.

Indeed, Goodin concedes that the decision to marry establishes matrimonial duties, but he argues that it does not specify the content of these duties; for this, he says, one must rely on his vulnerability model. He offers no justification for this view, which is not supported even by the most cursory view of marriage. Thus, two vulnerable people do not assume mutual obligations until the day they marry, and in most cases, the obligations end the day a divorce is granted. In fact, after a bitter divorce, spouses may derive a perverse satisfaction from their former partner's heightened emotional or financial vulnerability.

Public policy and the law recognize the contractual quality of the marriage bond. Whether a person is eligible for a share of the spouse's Social Security benefits depends not on one's dependency, but on how long one has been married; a rich person who had been married for 10 years receives a pension, while the poor person who had been married for only 9 years receives nothing. In most states, a person loses the right to support if he or she is even partially responsible for ending the marriage. A vulnerable adulterer is normally not entitled to alimony.

Vulnerability Is Not a Necessary Condition for Family Responsibilities

For Goodin (1985c, p. 779), "A is vulnerable to B if and only if B's actions and choices have a great impact on A's interests." Thus we have responsibilities for our families, because what we do affects their interests. Goodin's definition, however, is at odds with common usage in that it ignores the vulnerable person's condition in relation to other persons. According to *Cassel's Modern Guide to Synonyms & Related Words* (1971, p. 652), "*Vulnerable* always stresses a lack of protection against physical or mental harm." In ordinary usage, the vulnerable person is defenseless, assailable, unprotected, easily wounded, and susceptible to injury. This discrepancy bears on where one locates the source of family duties. Consider a millionaire son whose parents must decide whether to leave him their sizable fortune. According to Goodin's definition, the millionaire is vulnerable (his parents' choice will have a great impact on his interests), and accordingly, this vulnerability explains why the parents should decide in their son's favor. If we set aside the bequest's possible emotional ramifications, however, it seems bizarre to consider the millionaire as vulnerable. Even if he does not inherit a dime from his parents, he would not be defenseless or unprotected, and it is highly unlikely his parents fear that, in the absence of the bequest, their son would be susceptible to injury. In this case, whether the parents bequeath their estate to their son will depend not on his vulnerability, but on the affection and love they may or may not feel for him, or on their sense of family loyalty and continuity. Vulnerability, at least as the term is commonly used, is not a necessary condition for family responsibilities.

Goodin correctly recognizes that vulnerability is an important source for state responsibility for the poor, but he incorrectly believes that this responsibility would become more palatable if people would appreciate how it has the same basis as familial duties. His effort is bound to fail, because most people understand that family responsibilities are rooted in many complex considerations other than mutual vulnerabilities.

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New Right opponents of state welfare provision are anxious to shift the burden back to the private sector. The only thing they are keener on than "family responsibility" is "self-reliance," which at their hands comes to one and the same thing (Goodin, 1985b).

In my ethical defense of the welfare state, I attempted to turn the New Right fondness for family responsibility to strategic advantage (Goodin, 1985c; see also Goodin, 1985a, pp. 70-92). There, I argued that our responsibilities to provide for needy strangers derive morally from the same source as our responsibilities to provide for members of our own families. If so, then opponents of the welfare state can hardly repudiate the former while conceding (indeed, celebrating) the latter. To maintain an inconsistent attitude toward these two sorts of responsibilities, savoring the one while shunning the other, New Rightists would have to resist my argument about their shared moral basis in vulnerabilities.

Obler seems to concede that responsibilities toward needy strangers might derive from their vulnerability to our actions and choices, but he argues that responsibilities toward family members derive from different sources; vulnerability is neither a necessary nor a sufficient condition of our having such responsibilities. Furthermore, if this proposition is to do the work cut out for it in the New Right's larger argument against the welfare state (and it is unclear whether Obler wants to join them in that argument), this different basis must be such as to make family responsibilities substantially and systematically stronger than responsibilities to aid needy strangers based on vulnerability.

To differentiate family responsibilities from broader social ones, Obler points out that the former are assumed voluntarily, infused with affection born of biology, and sustained by reciprocal caring. This may be characteristically true of most family relationships, but is not necessarily true of any of them, and, at least in part, can never be true of some of them. The filial relationship cannot be voluntarily assumed, yet surely children are morally (and were, until recently, legally) responsible for caring for their aged parents. Obler concedes that example, saying that affection born of reciprocity can substitute for voluntary assumption of responsibilities. Thus, Obler seems to suppose that these factors can work independently to generate moral responsibilities.

Recall that Obler's self-defined task is to explain the "moral distinctiveness" of family responsibilities. To do that, he must invest each of the factors he nominates with some independent moral importance. Then, and only then, will they give rise to family responsibilities directly, in and of themselves. Then, and only then, will he have succeeded in offering an alternative account of the moral basis of family responsibilities.

While saying much to explain why those factors might matter psychologically, Obler says virtually nothing to explain why any of them should matter morally. In "reciprocal caring," it cannot be the reciprocity alone that matters. If so, children who leave home early would owe less to their aged, needy parents, and once they repaid their debts their responsibilities would end. Neither is true. In "affection born of biology," the biological link, although perhaps crucial psychologically, cannot possibly carry any moral clout. As for the "caring" and "affection" that run through both of these explanations, surely it cannot be right to say that we have moral responsibilities with respect to—and, self-assumed obligations apart, only with respect to—people toward whom we harbor warm feelings. Again, it may be good psychology, but the morals of the matter are something else.

The only really credible counterproposal is Obler's suggestion that family responsibilities are morally distinctive because they are (usually) assumed voluntarily. What it is about voluntary assumption that is morally special, Obler does not say. Others, however, point to personal autonomy, the will binding itself, etc. However, our responsibilities to children conceived accidentally—through leaky condoms or diaphragms, for example—are no less strong than our responsibilities to children whose existence we have fully willed. Furthermore, if the will binding itself is what really matters morally, then people's wills ought to be allowed to converge on any marriage contract they like. In practice this is not the case; there are certain "essential incidents" of a marriage contract (such as the duty of support) that cannot be legally waived, even by mutual consent (Goodin, 1985c, p. 777). The point is not that having composed a contract yourself is a precondition for your being morally bound to honor it; rather, if consent (the will binding itself, etc.) is what matters morally, then what is made by consent should be changeable by consent, and if the marriage contract cannot be changed, even by mutual consent, then it is scarcely credible to claim that the real moral force behind the contract lay in consent (voluntary agreement, acts of will, etc.) in the first place.

Obler's phenomenology is fine. Family responsibilities are often (if not always) assumed voluntarily, infused with affection, and sustained by reciprocal caring; it certainly feels as if the moral importance of family responsibilities is somehow tied to these characteristics. None of them, however, is itself the ultimate source of morally-charged family responsibilities. Instead, all merely point to mechanisms by which vulnerabilities might be engendered, and any moral importance attaching to those considerations is derivative from the moral importance of protecting the interests of vulnerable others.

People who have entered into voluntary agreements with one another, or who are emotionally involved with one another, or who are in a relationship of reciprocity with one another, will naturally rely upon (and hence be vulnerable to) one another in various respects. It is the sheer fact of this vulnerability, rather than how the vulnerability came about, that imparts moral force to the responsibilities of the relationship. For proof, note that responsibilities arising even under voluntary agreements diminish virtually to the vanishing point in situations where the promisee is in no way vulnerable to the promisor's defaulting: courts award purely nominal damages for breach of contract in situations where the promisee is in no way relying upon the promised performance (Goodin, 1985c, p. 777). So too with divorce courts: they refuse awards of support to ex-spouses capable to supporting themselves, imposing them only where one ex-partner or a child was necessarily relying upon the other for provision of reasonable necessities (Goodin, 1985a, p. 77). It is the absence of reliance, rather than the mere dissolution of the contract, that relieves marriage partners of their obligations to one another.

People have "emotional interests" every bit as strong as their material ones (Goodin, 1985c, p. 779). The resulting emotional vulnerabilities largely explain the "responsibilities of gratitude" Obler says children owe non-neglectful parents (Goodin, 1985a, pp. 104-7). They also explain why it is better for parents to take principal responsibility for feeding their own children, rather than leave the task to richer neighbors (Goodin, 1985c, pp. 778, 785), and why Obler's millionaire parents ought (prima facie, at least) to bequeath their fortunes to an already-rich son: his feelings, if not his standard of living, would be hurt if they left the money to the servants instead.

Such emotional vulnerabilities, if strong, can give rise to responsibilities that are relatively strong, but not necessarily so strong that they should always trump responsibilities growing out of material vulnerabilities of a more ordinary sort. In the case of the already-rich heir, we can surely imagine a set of servants so penurious we would all agree that, on balance, the parents' stronger responsibility would be to give their money to the servants instead of to their own child: however badly his feelings would be hurt, their starving bellies would hurt even more. Emotional vulnerabilities thus count for something, but not everything. The responsibilities growing out of them do not systematically trump the responsibilities to aid needy strangers in the way that the "family first" policies of the New Right would require.

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RESEARCH NOTES

ARE SOCIAL BACKGROUND MODELS TIME-BOUND?

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In this research note I seek to determine whether a significantly predicting social background model for analyzing the votes of Supreme Court justices is time-bound. I argue that an affirmative result poses serious questions for past uses of such models, none of which has controlled for the possibility that time is a confounding variable. A model that significantly predicted the votes of the justices in the Court's 1903–1968 terms was constructed. Analysis with this model for two periods—from 1903 to 1935, and from 1936 to 1968—established that the model was not time-neutral. Appropriate theoretical implications are drawn.

In introducing Justice Harry Blackmun recently, Norman Dorsen (1985, p. 13), professor of Law at New York University, explained the alleged metamorphosis in Blackmun this way: "We must never forget that the boy is father to the man, that the seeds of the fully mature person are long embedded in his character. One need not embrace Freudian psychology to conclude that early experience and training will be reflected in later actions and decisions, and that flexibility and open-mindedness are themselves the product of what has gone before."[1] Dorsen's comment is about as concise a statement of the social background theory as could be penned, and it reminds one of Fred Rodell's remark about Roger Brook Taney: "There is not one of his decisions as Chief Justice . . . but can be traced, directly or indirectly, to his big-plantation birth and background" (Schmidhauser and Gold, 1963, p. 484).

The fundamental proposition underlying the social background models employed in the judicial area is that past social experience, broadly defined, significantly influences the decisions that judges will make when confronted with socio-legal conflicts in their courts. Although tests of this proposition with federal and state judges through the years have produced mixed results, a number of studies have reported interesting findings (Goldman, 1966, 1975; Heike, 1978; Nagel, 1961, 1962; Schmidhauser, 1959, 1961, 1962; Schmidhauser and Gold, 1963; Ulmer, 1962, 1973a; Walker and Hulbary, 1978). The most ambitious of these studies (Tate, 1981) has reported recently that from 70% to 90% of variance in the voting of Supreme Court justices in a 30-year period was accounted for by seven variables: political party identification, the appointing president, prestige of prelaw education, appointment from elective office, the region of

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appointment, extensiveness of judicial experience, and type of prosecutorial experience.[2] Yet, as impressive as these studies may be, skeptics have not been lacking (Adamany, 1969; Bowen, 1965; Grossman, 1967; Hensley and Dean, 1984). Some critics have reported negative results with social background models. Others have articulated theoretical weaknesses in the model. None, however, have explored what I consider to be a major shortcoming in past applications: the failure to control for the possibility that such models are time-bound.

In a good play, one act is as good as any other. Similarly, other things being equal, if the past experiences of judges explain their decisional behavior, these explanations should hold in any and all segments of the time span examined. The relationship between social backgrounds and the decisions of judges should not depend on the particular composition of courts, nor on any other variables for which time is a surrogate. If that is not the case, what has been offered as theoretical results may be merely descriptive of particular courts at particular points in time. In such an event, the ability of social background theory to account for judicial behavior would be undermined. A proper assessment of such models, therefore, requires attention to time as a possibly confounding factor. In the remainder of this paper, the question posed in the title is explored by (1) constructing a social background model that explains a significant percentage of the variation in justice voting, (2) examining the model to see if it is time-bound, and (3) assessing the implications of the results.

Research Design

Let us begin with the assumption that governmental authority is an attitudinal object, with justices differing as to whether such authority should be maintained when in conflict with individual liberty, and at what cost (Ulmer, 1973b; Ulmer and Stookey, 1975). Our first question is whether the support given governmental authority by Supreme Court justices has varied appreciably across justices in the Court's terms from 1903 to 1968.[3] Tables 1 and 2 provide an affirmative answer to that question.[4] Four versions of a social background model are hypothesized to account for the observed variation.[5]

Predictor Variables

To assess the constancy of the model, analyses are carried out for two different time periods: 1903–1935 and 1936–1968. In effect, we compare two equal time periods, each of which is longer than that examined by Tate (1981). The analysis is pursued via several predictor variables.

The first predictive factor is whether the justice is an only or first-born child. In the last 20 years the scholarly community has produced over 400 articles and dissertations on birth order (Weber, 1984). In general, this work suggests that social and personality development is in part a function of birth order, mediated of course by the experiences one has with other family members as a consequence of being the oldest, middle, youngest, or only child. Such experiences are thought by some to influence the choices of marriage partner and vocation, and to have something to do with level of achievement.

Weber has applied birth order analysis to 102 Supreme Court justices. He reports that first-borns "tend to imbibe parental norms to a greater extent than later children, are conscientious, *anxious to please authority*, ambitious, and high achievers. . . . They . . . either prefer and/or have a high tolerance for tightly structured situations" (Weber, 1984, p.

Table 1. Supreme Court Justices' Support for the Government in Government vs. Underdog Cases, 1903–1968 Terms, by Court, Individual Justice, and Level of Government

	State and Local Government				Federal Government			
Justice	Court Support (%)	Justice Support (%)	Above or Below Court	*N*	Court Support (%)	Justice Support (%)	Above or Below Court	*N*
Harlan I	73.4	71.8	–	323	69.1	58.0	–	217
Fuller	71.9	69.0	–	288	68.5	54.6	–	181
Brewer	71.5	66.1	–	260	64.5	58.6	–	162
Brown	73.1	66.3	–	119	61.3	51.2	–	80
White, E.	72.5	70.8	–	768	67.4	65.9	–	613
Peckham	72.4	65.4	–	243	65.3	59.3	–	150
McKenna	71.3	69.9	–	916	65.9	65.4	–	831
Holmes	70.4	71.9	+	1273	65.6	63.3	–	1292
Day	72.3	70.4	–	812	68.1	64.6	–	678
Moody	71.3	65.8	–	164	77.8	53.5	–	99
Lurton	76.4	72.2	–	216	72.0	58.0	–	200
Hughes	71.8	69.6	–	831	66.6	63.2	–	1012
Vandevanter	70.1	67.4	–	1283	64.6	61.9	–	1466
Lamar, J. R.	74.0	67.8	–	246	71.6	57.2	–	215
Pitney	72.0	71.1	–	478	67.8	64.3	–	460
McReynolds	69.2	64.6	–	1255	64.2	58.0	–	1474
Brandeis	69.8	73.5	+	1086	64.1	64.7	+	1353
Clarke	72.7	73.0	+	256	69.7	62.7	–	290
Taft	66.7	63.8	–	423	64.9	61.7	–	544
Sutherland	69.1	66.3	–	782	63.4	57.9	–	996
Butler	68.5	63.5	–	840	63.3	58.1	–	1084
Sanford	65.5	63.2	–	348	65.0	59.3	–	448
Stone	68.1	70.6	+	926	65.2	65.7	+	1421
Roberts	67.1	65.1	–	629	64.7	57.4	–	1005
Cardozo	71.2	74.4	+	309	61.3	68.3	+	354
Black	47.1	44.6	–	1039	64.2	58.5	–	1882
Reed	60.4	62.3	+	523	66.3	68.1	+	1163
Frankfurter	56.7	57.3	+	624	65.2	65.7	+	1430
Douglas	45.1	30.1	–	956	64.0	50.4	–	1727
Murphy	60.4	55.2	–	288	67.9	61.5	–	663
Jackson, R.	58.6	55.8	–	324	65.3	55.3	–	685
Rutledge	58.3	48.1	–	187	66.0	61.1	–	435
Burton	56.0	61.4	+	298	62.2	65.2	+	719
Vinson	59.1	57.5	–	186	64.1	63.3	–	395
Minton	58.3	65.4	+	139	65.2	68.7	+	342
Clark	40.1	51.7	+	529	62.3	70.6	+	893
Harlan II	31.8	53.7	+	525	60.8	61.9	+	739
Warren	33.3	25.4	–	547	61.6	54.1	–	829
Whittaker	46.8	50.3	+	141	62.5	58.6	–	283
Stewart	31.5	41.4	+	466	63.0	60.6	–	592
White	27.5	42.3	+	352	65.1	73.1	+	358
Goldberg	20.1	18.6	–	134	65.3	54.6	–	150
Fortas	27.3	25.2	–	194	57.3	57.4	+	178
Marshall	32.2	28.0	–	121	72.6	57.5	–	73
Brennan	33.2	25.4	–	515	61.8	55.3	–	712
Byrnes	—	—	—	—	65.9	64.6	–	82

Table 1 (continued)

	State and Local Government				Federal Government			
Justice	Court Support (%)	Justice Support (%)	Above or Below Court	*N*	Court Support (%)	Justice Support (%)	Above or Below Court	*N*

Table 2. Supreme Court Justices' Support for the Government in Government vs. Criminal Cases, 1903–1968 Terms, by Court, Individual Justice, and Level of Government

	State and Local Government				Federal Government			
Justice	Court Support (%)	Justice Support (%)	Above or Below Court	*N*	Court Support (%)	Justice Support (%)	Above or Below Court	*N*
White, E.	92.9	91.4	–	70	73.2	69.0	–	71
McKenna	92.9	92.9	—	85	69.3	67.0	–	88
Holmes	90.4	92.5	+	135	67.1	63.3	–	161
Day	93.3	92.0	–	75	75.7	71.4	–	70
Vandevanter	89.0	86.6	–	127	63.7	62.5	–	179
McReynolds	81.8	83.3	+	132	59.7	58.0	–	186
Hughes	75.6	75.6	—	74	63.5	62.7	–	118
Brandeis	87.0	86.0	–	115	59.7	56.8	–	176
Taft	89.1	89.1	—	55	58.1	58.0	–	62
Sutherland	86.2	84.0	–	94	61.2	58.9	–	139
Butler	83.3	81.2	–	96	59.7	55.5	–	144
Stone	65.0	68.5	+	143	55.8	60.1	+	206
Roberts	58.3	67.7	+	96	53.6	53.5	–	153
Black	32.3	26.6	–	401	47.0	32.3	–	421
Reed	45.2	58.9	+	146	54.2	66.5	+	212
Frankfurter	43.7	47.2	+	199	50.0	46.7	–	312
Douglas	31.8	11.4	–	393	45.9	27.4	–	405
Murphy	41.6	23.5	–	89	47.5	26.7	–	101
Jackson, R.	50.5	60.9	+	105	52.7	48.8	–	129
Rutledge	44.4	22.2	–	63	54.1	40.5	–	74
Burton	50.5	67.8	+	109	52.1	67.8	+	165
Vinson	57.6	66.6	+	66	57.3	70.6	+	75
Clark	32.2	49.1	+	236	46.6	65.4	+	249
Harlan II	25.0	51.1	+	268	41.2	61.9	+	226
Warren	24.4	14.7	–	272	43.0	27.7	–	249
Whittaker	38.2	50.9	+	55	47.4	57.8	+	95
Stewart	22.6	37.1	+	234	41.1	54.7	+	168
White, B.	20.9	41.8	+	191	37.2	54.6	+	86
Fortas	22.3	17.3	–	121	35.1	40.3	+	57
Brennan	24.7	15.8	–	259	41.0	28.7	–	212
Sanford	—	—	—	—	55.6	51.8	–	54
Minton	—	—	—	—	62.8	75.6	+	78
Goldberg	16.9	12.3	–	65	—	—	—	—
Marshall	19.4	17.9	–	67	—	—	—	—

Note: Cases involving litigants characterized as black, alien, subversive, or labor union members are excluded from the data.

561, italics mine). Although controversially, it has been contended that first-borns, since they tend to be conservative and authoritarian, do not empathize with underdogs.

According to Weber, first-born Supreme Court justices, 56 in number, significantly exceed the expected number of 38—the approximate percentage of first-borns in the general population. Using Tate's ranking of 25 justices on support for civil liberties, he also reports that no significant correlation was found between birth order and support for civil liberty claims. However, I have calculated the mean rank for first-borns and compared it to the mean rank for those who were middle- or last-born. The respective means were found to be 15.33 for first-borns and 10.84 for non-first-borns. Consequently, we surmise that first-born Supreme Court justices will significantly favor government (authority) over underdog and criminal litigants in the Court's cases matching the government against such litigants.

The second factor to be considered is whether a justice's father was at any time a state or federal judicial officer, a legislator, or an executive officer—elected or appointed. This choice is based upon the assumption that government service promotes a favorable attitude toward government and its needs. It is consistent with Tate's (1981) finding that justices who had been prosecutors were more conservative than nonprosecutor judges, and with my earlier finding that federal administrative experience was positively related to support for government in criminal cases (Ulmer, 1973a). Our expectation is that Supreme Court justices whose fathers had governmental experience imbibe pro-government sentiments, and thus significantly favor the government in cases in which it is matched with underdog and criminal litigants.

The third and last predictor is the political party affiiation of the justices. One may note earlier findings that, in limited settings, Democratic judges have significantly favored the claims of the injured and the unemployed (Nagel, 1961, 1962; Ulmer, 1962) and of the defendant in criminal cases (Nagel, 1961). Democratic judges have also been found to be more likely than Republican judges to find a constitutional violation in criminal cases (Nagel, 1961) and to be more liberal than Republican judges (Goldman, 1975; Goldman and Jahnige, 1985). At the Supreme Court level, Tate has shown that party is a significant predictor of the justices' votes in civil liberty cases, which means, among other things, that Republicans favor government in such cases while Democratic justices tend to favor underdog and criminal litigants.

Such findings, of course, do not tell us whether party shapes attitudes or attitudes are shaped by party choices, or whether other experiences in life determine attitudes and party choice. We surmise that, unlike some experiential variables, party choice is a function of multiple experiences, and thus a surrogate for the aggregate of attitudes these multifarious experiences produce. It may not be strictly logical, therefore, to argue that party "causes" or influences judicial votes. It is logical to view the variable as a surrogate for background factors that, for the present, remain unidentified. Our general expectation is that when government is matched against underdog and criminal litigants, Republican justices will favor government significantly more often than Independent justices, and Independents more often than Democrats.

These predictors are employed with four versions of the same model, each of which incorporates the same dependent variable—support for government. Table 3 summarizes and distinguishes the four versions of the model (hereinafter referred to as Models 1, 2, 3, and 4). In each case, we suggest that the three predictors will account for a significant portion of the

Table 3. Four Models for Predicting Variation in Support for Government from Backgrounds of Selected Supreme Court Justices, 1903–1968 Terms

Model	Predictors	Criterion
1. Cases matching state or local government against underdogs	Father a state officer; party of justice; justice a first-born	Support for state or local government
2. Cases matching federal government against underdogs	Father a federal officer; party of justice; justice a first-born	Support for federal government
3. Cases matching state or local government against non-underdog criminals[a]	Same as 1	Support for state or local government
4. Cases matching federal government against non-underdog criminals[a]	Same as 2	Support for federal government

[a]Cases involving criminals characterized as black, alien, subversive, or labor union members are excluded.

variation in the support the justices give state/local or federal governments, as appropriate.

Dependent Variable

To construct a measure of the dependent variable, we first determine the support each justice gave to government during his entire career by taking support for government as a percentage (*A*) of all cases falling in the prescribed category. A similar percentage (*B*) is taken for each Court (the majority vote) in the same time period. The ratio of these percentages (*A/B*) is then calculated for use as the criterion in an ordinary least squares regression (OLS) analysis.[6]

For each of the models summarized in Table 3, the first question is whether the three independent variables are effective predictors of the criterion appropriate for each model. An ordinary least squares analysis is then applied to each of the four models for the Court's terms from 1903 to 1968, using only those justices who participated in at least 50 cases on the dimension being analyzed.

Findings

The results for Models 1, 2, and 4 may be quickly summarized. These models explain an insignificant and miniscule proportion of the variance in the justices' voting behavior. Thus, the expectations expressed for these models are not realized.

For Model 3, the picture is somewhat different. Several regression analyses have been run for this model. The equation for the first analysis is

$$y = a + b_1 FAST_1 + b_2 FB_2 + b_3 PARTY_3 + e, \qquad (1)$$

where *y* is the ratio of the percentage of support for government by the justice and the Court (justice/Court), and *FAST, FB,* and *PARTY* refer to the justice's father as a state officer, the justice as first-born, and the justice's party affiliation, respectively. Each of the first two variables is recorded as 1 if present and 0 if absent. *PARTY* is recorded as 2 for Republicans, 1 for Independents, and 0 for Democrats. Due to coding procedures, the signs of all regression coefficients are expected to be positive. The results of this and related analyses are provided in Tables 4, 5, and 6.

Table 4 shows that the model explains a significant percentage of the variance in support for government. The question that must be addressed, however, is

Table 4. OLS Regression of Father as State Officer, Justice as First-Born, and Political Party of Justice on Support for Government, 1903–1968 Terms

Variable	Regression Coefficients	Standard Error	Significance
Father as state officer	144.97	42.76	$p < .01$
Justice as first-born	12.85	20.45	n.s.
Party of justice	5.94	10.80	n.s.
Constant = 92.70			
$R^2 = .359$			

whether this finding is valid or merely an artifact of the time dimension used in delimiting our data base. To assess that possibility, we need to know whether the effect of time on slopes b_1, b_2, and b_3 undermines the inferences to be drawn from the first analysis. The equation needed is

$$\begin{aligned} y = a &+ b_1 FAST + b_2 FB + b_3 PARTY \\ &+ b_4 TIME + b_5[(FAST)(TIME)] \\ &+ b_6[(FB)(TIME)] \\ &+ b_7[(PARTY)(TIME)] + e, \qquad (2) \end{aligned}$$

where *FAST*, *FB*, and *PARTY* are as defined for equation (1); *TIME* is a dummy variable that equals 0 for the 1903–1935 terms and 1 for the 1936–1968 terms; and the model includes several multiplicative terms involving *TIME*. This model allows for the possibility that the effects of *FAST*, *FB*, and *PARTY* on a justice's support for government during the first time period, when $TIME = 0$, differ from the effects in the second period, when $TIME = 1$. In particular, b_1, b_2, and b_3 represent the slopes of the effects of *FAST*, *FB*, and *PARTY*, respectively, on support for government during the pre-1936 period. In contrast, b_5, b_6, and b_7 measure the differences in slopes of effects from the pre-1936 period to the post-1935 period. For example, b_5 represents the extent to which the slope of the effect of *FAST* on support for government is greater in the second time period than in the first.

Table 5 portrays the unstandardized

Table 5. OLS Regression of Father as State Officer, Justice as First-Born, Political Party of Justice, and Time on Support for Government, 1903–1968 Terms

Variable	Regression Coefficients	Standard Error	Significance
Father as state officer	-2.25	44.69	n.s.
Justice as first-born	2.16	24.10	n.s.
Party of justice	1.34	13.18	n.s.
Time	1.65	28.18	n.s.
[(*Fast*)(*Time*)]	289.97	64.30	$p < .001$
[(*First-born*)(*Time*)]	-10.46	31.65	n.s.
[(*Party of justice*)(*Time*)]	16.33	17.20	n.s.
Constant = 97.57			
$R^2 = .729$			

Table 6. OLS Regressions of Father as State Officer, Justice as First-Born, and Party of Justice on Support for Government, 1903–1935 and 1936–1968 Terms

Variable	Regression Coefficients	Standard Error	Significance
1903–1935			
Father as state officer	-2.25	5.10	n.s.
Justice as first-born	2.16	2.75	n.s.
Party of justice	1.34	1.50	n.s.
Constant = 97.57 R^2 = .184			
1936–1968			
Father as state officer	287.72	58.33	$p < .001$
Justice as first-born	-8.29	25.90	n.s.
Party of justice	17.67	13.95	n.s.
Constant = 99.22 R^2 = .723			

slope coefficient estimates for the equation. The most dramatic differences in effects across the two time periods is for the variable *FAST*. *FAST* has almost no effect on government support for the pre-1936 period, as its slope coefficient is almost zero (−2.25). But the slope coefficient for *FAST* during the post-1935 period is 289.97 units higher than the slope in the first time period, and this difference in slopes is statistically significant, as the coefficient for [(*FAST*)(*TIME*)] is significant at the .001 level. While there are also differences in the slopes of the effects of *FB* and *PARTY* on support for government across the two time periods, the standard errors of the coefficients for [(*FB*)(*TIME*)] and [(*PARTY*)(*TIME*)] indicate that differences in slopes are not statistically significant.

In Table 6, what are essentially the results of the analysis in Table 5 are put in a different form for emphasis. It will be noted that the OLS regression for the Court's 1903–1935 terms accounted for 35.9% of the variance when time was ignored, but 72.9% when time was incorporated in the analysis. In Table 6 the regression is run first for the period 1903–35, and compared with the results of a regression analysis run on the 1936–68 terms. The significant disparity between variance accounted for in the first period (18.4%) and that accounted for in the second (72.3%) underscores the impact of time period when the effect of *FAST, FB,* and *PARTY* on voting ratios is determined. One can only infer that in the initial 66-year analysis of support for government, the impact of social background factors on the justice's voting decisions was time-bound.

Summary and Discussion

This paper asks whether social background models for explaining the votes of Supreme Court justices are time-bound. Adopting a research design that made it unlikely that social background would explain variation in justice voting across a 66-year period, a three-predictor model was identified that accounted for a statistically significant percentage of the variance: 35.9%.

Incorporating a time dimension in the

analysis, however, we found a statistically significant difference in the impact of the three predictors in the Court's 1903–1935 terms when compared to its 1936–1968 terms. Indeed, when regression analyses were run separately for these two periods, the model accounted for 72.3% of the variance in the second period, but only 18.4% in the first. The result for the 1936–1968 terms compares favorably with the most successful use of social background models in studying justice voting. Such a result would be eminently publishable by past standards. The result for the first period, on the other hand, would not likely be considered worth reporting to the discipline. The conclusion to be drawn, in any event, is that the social background model examined in this paper is not time-neutral.

Although our results do not speak directly to past applications of social background models, our analyses suggest the possibility that all past applications in the judicial area have been time-bound—that is, they work only for particular sets of judges at particular points in time.[7]

If this fact were established, it would tend to undermine a major tenet of social background theory, namely, that social background is a relatively unattenuated program for future behavior. At least it would seem essential that future users of the model explore nonbackground variables that possibly condition the impact of social attributes on subsequent behavior—variables for which time is possibly a surrogate.

None of our findings establishes necessarily that social background models in general are time-bound, although they do suggest that such a question must now be raised about any such model that claims to explain Supreme Court decisions. The reasons are two. First, we have investigated particular sets of cases in particular time periods. Our results may not hold for other cases or other time spans. Second, and more important, the failure of our models or others to predict voting behavior does not establish that social background has nothing to do with shaping attitudes toward objects. Certainly it does not establish that attitudes toward objects are irrelevant to voting behavior, for we are left with the possibility that, while social backgrounds do shape attitudes, and attitudes influence behavior, held attitudes will not always be reflected in behavior that would be consistent with them. Just as a racist, if appropriately circumscribed by law, peer pressure, likelihood of censure or sanction by relevant others, etc., may withhold racist behavior, a Supreme Court justice whose political loyalties would normally induce him to support the president who appointed him may not do so given significant countervailing pressures. This point is illustrated, perhaps, by Chief Justice Burger's vote in *United States v. Nixon* (1974), and that of Oliver Wendell Holmes in the *Northern Securities Case* (1904).

All this suggests the need for new research addressed to (*a*) the direct relationship between social attributes and attitudes, and (*b*) the conditions under which established attitudes will lead to specific behavior among judges. The second of these can only be addressed satisfactorily after the first has been established. To reach solid inference about the first for Supreme Court justices is admittedly a difficult challenge.

I would distinguish attribution of attitudinal objects from inferences about such objects derived by such reliable procedures as interviews and survey instruments. The latter investigatory tools are simply not available for assessing attitudes in the Supreme Court. A possible strategy here would be to investigate the validity of social background linkages to attitudes by focusing on lower court judges who will give interviews and, possibly, even respond to questionnaires. Establishing the validity of the general model in that rather direct fashion would presumably then lend greater credence to more indirect efforts to account for the behavior of Supreme Court justices by examining their social backgrounds.[8]

Notes

I wish to express my appreciation to Jim Gibson of the University of Houston for hepful comments on an earlier draft of this paper, and especially to my colleague Bill Berry, whom I consulted on the methods section of the paper. Any deficiencies remaining are the sole responsibility of the author.

1. Dorsen's comment is only a recent manifestation of a belief held by many who have studied Supreme Court justices over the years. Murphy's biographer (Howard, 1968) noted that Justice Murphy was especially attentive to claims based on unorthodox religious belief, and that Murphy's attitudes toward obscenity, family law, and divorce were influenced by his religion. Sutherland's biographer suggested that Mormon influences solidified Sutherland's Spencerian ideology (Pascal, 1951). Alpheus Mason (1956) notes the influence of an Amherst teacher on Justice Stone's philosophy. McGrath (1963), writing about Waite, concluded that the justice's frontier experiences in Ohio prior to coming to the Court left him with a faith in people that translated into support for popular legislatures once Waite became a judge. Systematic study of the relationship between the social background of judges and the decisions they make can be traced as far back as the thirties (Ewing, 1938; Mott, Albright, and Simmerling, 1933). However, political science did not exhibit much interest in this area until two decades later. In a five-year period from 1959 to 1963, judicial scholars published half a dozen papers that rekindled the interest in social background as an explanation for variation in the decisions of judges. That interest has continued to the present.

2. Jim Gibson has observed (in a letter to the author) that since the relationship between social background factors and justice behavior is mediated through judicial attitudes, the relationship between the two is always measured in attenuated form, and, because of indirect effects, may actually be stronger than the measurements indicate. Applying this thought to Tate's (1981) model underscores the remarkable results he achieved with seven predictors.

3. The choice of such a relatively long time span —66 years—was based on two considerations. First, the most successful attempt to explain Supreme Court decision making via the social attributes of the justices, that of Neal Tate, covered the most extensive period to date—a span of 30 Court terms. Second, and more important, a period of 66 years enables us to investigate the potential of social attribute theory to account for behavioral variations over both short and long terms.

4. A mere glance at these tables makes it quite clear that support for government in each case varies dramatically across the justices taken in the aggregate. In Tables 1 and 2, the support percentages run from 18.6% (Goldberg) to 73.5% (Brandeis); 50.4% (Douglas) to 73.1% (White); 12.3% (Goldberg) to 92.9% (McKenna); and 26.7% (Murphy) to 75.6% (Minton).

5. Our four sets of cases are delineated in such a way as to control for certain possibly confounding factors. Set 1 consists of cases matching state or local governments against underdogs, while Set 2 makes the same match with the federal government as a litigant. Underdogs in these cases are litigants characterized as black, alien, criminal, subversive, or labor union. Set 3 matches federal government and non-underdog criminals—that is, criminals not characterized as black, alien, subversive, or labor union—while Set 4 makes the same match for state and local governments.

6. By using the ratio A/B as the criterion, we control to some extent for (1) external social and environmental influences playing upon the justices in a given era, (2) the legal strength of the case, and (3) the small-group influences emanating from the particular makeup of the Court at any particular time. Earlier studies have not employed such controls, thereby leaving unanswered questions about the impact of exogenous variables on levels of explained variance. The use of the ratio employed here should make it more difficult to obtain significant levels of explained variance; that is, to the extent that these control factors are related to the independent variables, the deck is somewhat stacked against affirming model predictions.

7. In his study of 25 justices over 32 terms in civil liberty cases, Neal Tate (1981) was able to account for 87% of the variance with seven variables. Our analysis of the Court's 1936–68 terms with only three predictors accounting for 72% of the variance is clearly in the same "ballpark." Given that our 33 terms overlapped 23 of the terms studied by Tate, and assuming that attitude toward civil liberty claims is the obverse of attitude toward governmental authority, it is possible that an application of our Model 3 to the 32 terms studied by Tate would produce results similar to those we have reported for the slightly different period. Such a finding would suggest that justices serving in this era were products of their social backgrounds, and that they were willing to allow the attitudes deriving from those backgrounds to affect their voting behavior significantly.

8. For a more extensive treatment of some of the matters discussed in this research note see Ulmer (1985).

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PARTISANSHIP AND GROUP SUPPORT OVER TIME: A MULTIVARIATE ANALYSIS

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We propose measuring group support for political parties by means of multivariate techniques that have become standard in other areas of political behavior. This approach yields improved insights into the marginal difference made by membership in each group and into the nature of a party's support coalition. As an example of this approach, we analyze the Democratic coalition since 1952. Our results differ from those of previous studies in a number of ways. Most significantly, differences with respect to the strength and timing of partisan changes lend support to Carmines and Stimson's conclusion that a realignment centering on race occurred in the mid-1960s. Our findings also indicate that the Democratic party is no longer so dependent on a few groups, as it was in the 1950s, but is now almost equally dependent on six groups.

Studies of changes in party coalitions (Axelrod, 1972, 1982; Petrocik, 1981) have focused either on overlapping groups, such as blacks and the working class, or on exclusive groups such as white, middle-class Protestants. Neither approach is adequate. Use of overlapping groups makes it impossible to tell which characteristics are the crucial determinants of party support. Reliance on exclusive groups largely predetermines the outcome, because the critical groups have to be defined a priori.

We propose a new and more revealing means of measuring group support. By analyzing the partisanship of individual group members with multivariate methods similar to those applied elsewhere in the study of political behavior, we provide improved insights into the marginal difference made by membership in each group and into the makeup of a party's support coalition. As an example of our approach, we examine the Democratic coalition over the past three decades. Our results strengthen conventional wisdom about individual partisan changes by blacks and southern whites, but they differ significantly with respect to the strength and timing of changes by these and other groups. In addition, our findings lend support to Carmines and

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Stimson's (1982, 1984) conclusion that a realignment centering on race occurred in the mid-1960s.

Party Identification: Measurements of Changes in Group Support

Multivariate analysis has become a common practice in studying most kinds of political behavior. The logic behind this development requires as well the adoption of a multivariate approach for the study of group support of the parties. Here we will rely on multivariate logit, because our dependent variable—claiming Democratic identification—is dichotomous, and because logit better reflects our theoretical expectations. With logit, the partisan impact of a group membership varies across individuals, depending upon the other group memberships of those individuals: a group membership has little impact on those who are otherwise almost certainly Democratic or extremely unlikely to be Democrats; it has its greatest effect on those in the middle.

In the present application we focus on partisanship over the period from 1952 to 1984, with special emphasis on the extent to which the New Deal coalition supporting the Democratic party has deteriorated. We concentrate on party identification as the more durable indicator of partisanship. Thus we estimated a total of nine equations—one for each of the nine presidential elections from 1952 to 1984.[1]

In order to make the comparisons over time meaningful, the same exclusively group-based model is applied to each presidential election. The model includes the core elements of the New Deal coalition: native southern whites (11-state definition), blacks, Jews, the working class, union members (in the household), and Catholics. A variable for gender is also included. Other variables were considered in the initial analysis but were dropped, because they had statistically insignificant relationships to Democratic partisanship.[2]

The Results

Overall and Incremental Effects of Group Memberships

Axelrod (1972, 1982) found that between 1952 and 1984 three groups had decreasing loyalty to the Democrats: Catholics, southerners, and individuals in households with union members. The poor, blacks, and those residing in the central cities of the nation's 12 largest metropolitan areas showed increasing Democratic loyalty. Petrocik (1981) reported that all groups except blacks shared a marked tendency toward independence and a diminished partisan preference; blacks became much more strongly Democratic.

Our results confirm the sharpest of these movements—especially of blacks and southern whites—but they differ in other respects. First of all, once other variables are taken into account, being poor and residing in metropolitan areas did not incline individuals toward Democratic identification (i.e., they did not achieve statistical significance) in the 1952-1984 period. The tendency of the poor and of metropolitan residents to support the Democrats can be accounted for by their other characteristics. Two indicators of status—education and income—were also not related to Democratic identification, perhaps because subjective identification with the working class better revealed the connections of status to partisanship.

Given the nonlinearity of logit, the original coefficients (not shown) are difficult to interpret. Transforming them into probability values, however, yields two summary values of special significance—the overall predicted mean probability of being a Democrat for each

Table 1. Overall Probability of Democratic Identification for Group Members, 1952–1984

Group	Year								
	1952	1956	1960	1964	1968	1972	1976	1980	1984
Black	.53	.50	.45	.79	.85	.68	.70	.70	.65
Catholic	.56	.51	.65	.61	.54	.50	.50	.42	.42
Jewish	.70	.60	.50	.52	.54	.52	.57	.69	.59
Female	.47	.43	.49	.59	.51	.43	.42	.42	.40
Native southern white	.75	.70	.72	.71	.52	.50	.52	.46	.42
Union household	.54	.50	.57	.67	.53	.45	.47	.44	.46
Working class	.54	.48	.51	.64	.55	.45	.46	.44	.42

Note: Cells are the mean of the predicted probabilities of Democratic identification for all group members in each year.

group, and the incremental probability difference each group membership makes.[3]

Table 1 presents the mean predicted probability that a group member would claim Democratic identification in each of the nine presidential election years—that is, the frequency of Democratic identification in each group before imposing any controls for other group memberships. Even these initial figures show the plight of the Democrats. Comparisons of the chronological endpoints, for example, show that only for blacks was the probability of Democratic identification significantly increased. White southerners registered the strongest decline, with most of the decline coming in the 20-point drop between 1964 and 1968, and in the 10-point drop between 1976 and 1984. The probability of Democratic identification for Catholics, females, individuals in union member households, and self-identified members of the working class, although peaking in 1960 or 1964, later declined to levels below those of the 1950s.

The incremental impact of a particular group membership is isolated in Table 2. The strongest movements over the years are equally apparent in these figures. The black increase, the southern white decrease, and the Jewish decline and recovery are all evident in Table 2, and

Table 2. Incremental Probability of Democratic Identification for a Particular Group Membership, 1952–1984

Group	Year								
	1952	1956	1960	1964	1968	1972	1976	1980	1984
Black	.17	.17	.09	.34	.47	.36	.40	.38	.35
Catholic	.20	.18	.29	.17	.17	.19	.20	.09	.12
Jewish	.38	.32	.18	.17	.23	.26	.34	.41	.34
Female	.00	−.03	.04	.04	.04	.06	.06	.08	.06
Native southern white	.43	.41	.41	.33	.17	.20	.24	.17	.13
Union household	.10	.11	.13	.14	.07	.07	.10	.08	.11
Working class	.12	.09	.09	.12	.09	.07	.08	.07	.06

Note: Cells are the average of the difference, for each group member, between the individual's predicted probability of Democratic identification and what the individual's probability would have been without the effect of the group membership.

are of about the same magnitude as in the overall results.

Significantly, however, the patterns are not identical to what has been observed using other approaches. Axelrod (1972), for example, found that in 1952 blacks were far more likely (by 38%) than the general population to vote for Democrats. That Democratic edge declined until 1964, when it rebounded to the same level as in 1952. Our results are not a direct contradiction, because they involve slightly different questions (i.e., the marginal impact and partisan identification), but they suggest that being black did not contribute strongly to being Democratic until 1964; before that, it had less marginal impact than, for example, being Catholic. In 1964 and later, however, the effect of being black contributed much more significantly to a Democratic identification than it had at any time in the 1950s.

Similarly, the impact of being Jewish declined between the 1950s and 1960s, but unlike Petrocik's (1981) trend, our results show a rebound, even by 1972, when his series ended. This trend continued through 1980, leaving the marginal impact very nearly what it was at the start.

Though smaller and therefore less noticeable, a contrast with earlier results is also apparent among Catholics. Axelrod (1972) and Petrocik (1981) found the contribution of Catholics declining by 1972 and 1968, respectively. Our data suggest that, except for the special circumstances of 1960, being Catholic stimulated a Democratic identification to about the same degree until 1980.[4]

The sharpest contrast suggested by our approach occurs for members of union households. Here both Axelrod's earlier results and our overall probabilities suggest a decline in Democratic proclivities of about five to eight percentage points. In contrast, the incremental push that comes from this characteristic appears to have changed very little over the last three decades. This difference may be identifiable because we are able to separate the effect of being working class from that of being in a union household. Since many union members consider themselves working class, and since there was a drop in the marginal impact of this variable, exclusion of the working class measure might erroneously indicate a decline in the effect of being in a union household.

Observe also the contrasting images created by the overall versus incremental probabilities for women. Females are less likely to identify themselves as Democrats in the 1980s than they were in the 1950s; at the same time, being female has given an increasingly larger boost to Democratic identification. Note, however, that this increment is not simply a Reagan phenomenon. Marginal female support for the Democrats goes all the way back to 1960 and has been slowly but steadily increasing since then. Interestingly, the only reversal in the pattern is in the latest Reagan election. It remains to be seen whether or not this counterintuitive change continues.

In addition to our ability to distinguish overall and marginal effects and to control more effectively for multiple characteristics, another way of using our multivariate approach is to ask whether changes in the mix of demographic characteristics have had a significant effect on the probability of claiming Democratic identification. It is possible, for example, that the decline in the number of females identifying with the working class (60% in 1952; 50% in 1984) and other such changes can largely account for the observed changes in tendencies towards Democratic partisanship. Reliance on either overlapping or exclusive groups cannot easily address such possibilities. However, there is a straightforward way to study this question. We can apply the coefficients from a later election year to an earlier distribution of individual group membership and contrast these hypothetical results with the actual ones for the later year. Such a comparison conveys the degree of partisan change arising from shifts in the coefficients (that is, the partisan meaning of group memberships). By similarly varying the distribution of group memberships, one can gauge the degree of partisan change arising from shifting mixes of individual group memberships.

The two sets of coefficients for the endpoints 1952 and 1984 capture the partisan meanings of group memberships for those two years. What happens if the patterns of meanings for 1984 are applied to the 1952 data? The overall probability of Democratic identification for the groups is not appreciably different, whether one applies the 1952 coefficients to 1952 or 1984 data. The changing composition of groups made for very little partisan change. However, major differences in probabilities result from applying the 1984 coefficients to the data from 1952. Ignoring the signs, the mean change in probabilities for varying the data distribution is less than two points, while for varying the coefficients it is 14 points. Thus the shifts in the meaning of group memberships far outweigh the partisan implications of demographic changes. Changes in the partisan meaning of group memberships, not declines in reinforcing group memberships, account for the decreasing probabilities of identifying with the Democrats.

Group Support and the Democratic Coalition

Axelrod's (1972) analysis reveals the extent to which the Democratic coalition is made up of blacks, metropolitan residents, and so on. If one wishes simply to know a group's share of the Democratic coalition, Axelrod's approach has much to recommend it. However, studying groups within party coalitions—just as studying individual voting behavior—does not require us to treat the groups as if they were monolithic. Since most individuals have multiple group characteristics, removing a single characteristic from the mix need not mean that all members of that group desert the party. While Axelrod's findings reveal the coalition share each group composes, his figures cannot reveal, for example, how many southern whites would remain Democratic if being a southern white gave no nudge toward Democratic identification. Nor can they show what the size of the Democratic coalition would be if that Democratic propensity was lost. In contrast, our approach yields both of these results.

Such results are hypothetical, of course, but they are what the party strategist would most like to know. If the party stopped appealing to southern whites as such, what would that do to the party coalition? How many southern whites would remain Democratic despite the changing appeals, and how much of a loss would the party sustain?

Answers to these questions, along with comparison figures of the type that Axelrod generated, are given in Table 3. In the first two sections of the table we show the proportion of Democratic identifiers in the U.S. and, below that, the percentage of the Democratic coalition with a given group characteristic. This breakdown of the coalition is in terms of overlapping groups, making these percentages analogous to those presented for the Democratic vote by Axelrod. Not surprisingly (because of their size in the population), working class identifiers and females consistently make up the largest shares of Democratic identifiers. Blacks and Jews, despite their high Democratic loyalty, constituted the smallest shares of identifiers.

The results in the third section of the table show what would have happened to each group if the Democratic increment due to the group characteristic were removed. Females, for example, would

Table 3. Size and Composition of the Democratic Coalition, 1952–1984

	Year								
	1952	1956	1960	1964	1968	1972	1976	1980	1984
Proportion of Democratic Identifiers in the U.S.	47.3	43.7	47.3	52.9	45.5	40.3	39.5	39.3	38.0
Percentage of Democratic Coalition with a Given Group Characteristic[a]									
Black	10.8	9.8	7.9	13.6	16.8	16.2	18.3	19.2	19.2
Catholic	26.1	24.8	27.3	25.2	26.3	29.7	31.5	25.0	31.0
Female	53.8	53.8	54.8	56.2	58.8	59.7	61.6	60.9	61.3
Jewish	4.8	4.3	3.4	2.8	3.3	2.9	3.5	5.8	4.1
Native southern white	25.6	27.4	27.3	20.3	18.7	20.7	20.1	19.4	20.8
Union household	31.5	31.7	33.1	29.6	28.6	28.9	27.6	29.3	25.7
Working class	70.5	69.0	72.0	65.8	62.9	61.1	61.5	58.5	57.0
Percentage of Democratic Identifiers in Group Continuing to Claim Democratic Identification After Removing Democratic Tendency of Defining Group Characteristic[b]									
Black	68	67	81	56	44	46	43	46	46
Catholic	64	65	55	71	69	63	59	79	73
Female	100	106	92	94	93	87	85	82	86
Jewish	46	48	63	68	58	50	40	41	42
Native southern white	42	42	43	54	66	60	53	64	69
Union household	81	79	78	80	87	85	79	82	77
Working class	77	82	82	82	84	85	83	84	85
Relative Size (%) of Democratic Coalition After Removing Group Characteristic									
Black	96.6	96.8	98.5	94.1	90.5	91.3	89.4	89.6	89.7
Catholic	90.7	91.3	87.9	92.6	91.4	89.1	87.1	94.7	91.6
Female	100.0	103.4	94.8	96.0	95.2	92.3	90.6	88.8	91.3
Jewish	97.5	97.7	98.7	99.0	98.7	98.5	98.0	96.4	97.6
Native southern white	85.2	84.0	84.6	90.7	93.6	91.8	90.6	93.1	93.4
Union household	93.9	93.1	92.6	93.2	96.0	95.8	94.2	94.7	93.9
Working class	83.5	87.4	86.7	87.0	88.8	91.1	89.4	90.6	91.6

[a] Figures derived from taking the mean predicted probability of Democratic identification for a group in a particular year multiplied by that group's number of respondents, and dividing this product by the number of Democratic identifiers.

[b] Figures derived by recalculating the probabilities of Democratic identification without the effect of, say, working-class identification, then taking the mean of these probabilities for all respondents who claimed working-class status. The ratio of this revised mean probability to the mean probability that includes the effect of working class gives the ratio of the hypothetical size to the actual one.

have been the least affected. Despite the gender gap, Democratic appeals to women as such have been sufficiently few, and women's other characteristics sufficiently pro-Democratic, that even in 1984, 86% of the females who identified with the party would have continued to do so if there were no special push due to their gender. In contrast, less than half of the blacks and Jews would have continued to identify with the party if it lost its special appeal to them as blacks and Jews.

What effect would such changes have had on the party coalition? The last section of Table 3 shows that the Democrats used to depend more variably and more heavily on appeals to certain characteristics. The loss of a given group increment would have reduced the coalition by as little as 2-3% or by as much as 16%. Recently, however, the overlap among group memberships and the Democratic tendency of each membership has been such that the loss of appeal to any of the characteristics would result in a rather similar 6-10% loss in identifiers. This change is significant, but it leaves the Democrats no less vulnerable than before. The party is no longer so dependent upon a couple of groups, as it was in the 1950s, but is now almost equally dependent upon six.

Conclusion

Previous studies of the group basis of partisan identification define groups in overlapping or exclusive terms, concealing rather than revealing the net partisan impacts of an individual's multiple group memberships. This study employed multivariate logit to assess partisan change, measuring the incremental impact of individual membership in selected groups on Democratic partisanship.

In part, the findings reported here strengthen current knowledge about shifting group loyalties. Being Jewish or being in a union household pushes an individual in the direction of being a Democrat as much as it did in the 1950s. Being black or being female gives a greater push now than either did three decades ago. Catholics, native southern whites, and working-class members receive a lessened stimulus toward Democratic identification from memberships in these groups.

The possibility that demographic change—reduced reinforcement of partisan tendencies from multiple, overlapping group memberships—figured prominently in the observed Democratic decline was tested and found wanting. Changes in the partisan meaning of group memberships proved a far more potent link to the decline. We also noted that the contribution of the varying groups to the Democratic coalition was now highly similar, so much so that a loss of appeal to any one group would be a strong blow to an already weakened party.

In addition to strengthening current knowledge, the findings refine our understanding of the nature and timing of changes in Democratic support. The timing of changes by blacks and native southern whites—rapid shifts in the 1964-1968 period—suggest a racially-inspired shift in the group basis of the Democratic party consistent with Carmines and Stimson's (1982, 1984) thesis of a racial realignment (or at least a dealignment) of the parties at that time.

Finally, in the light of the findings reported here, journalistic and party obituaries for the New Deal coalition appear harsher than the reality warrants. Native southern whites had a strong, prolonged decline in their incremental partisan impact, but the milder declines for most groups, coupled with the increases for females, Jews, and blacks, suggest the New Deal group basis of Democratic identification may not be dazzling, but is far from dead.

One reason for the resilience of the New Deal coalition may turn on the observation that the arrival of a new partisan order does not require the obliteration of the old ties. Sundquist (1983, p. 17) notes that even in a new realignment, the old realignment patterns endure—reduced, no doubt, but still detectable, with the realignment process resembling a "collage of successive overlays." The New Deal group cleavage may be of this sort; it has not disappeared, but it may have been superceded by other cleavages. Traditional group partisan differences still endure, but new ones are now as prominent, or more so. Groups such as feminists, evangelicals, environmentalists, residents of the Sunbelt and Frostbelt, "yuppies," Hispanics, and workers in high-tech or smokestack industries suggest clusters of voters whose loyalties might now be eagerly sought. As for the New Deal coalition itself, it is clearly smaller than in the past. However, if the New Deal coalition is not alive and well, at least its corpse has yet to disappear.

Notes

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1. Altering the definition of Democratic partisanship to include leaners does not change any of our conclusions. When the same model is examined using Democratic presidential voting as the dependent variable, there are strong short-term fluctuations, but the trends essentially resemble those found for partisanship.

2. These were age, income, education, metropolitan residence, white migrants into the South, rural residence, blue-collar workers, white-collar workers, farmers, Protestant, Irish or Polish descent, and foreign-born parents. Multicollinearity had little effect on which variables remained in the equation. In fact, two of the most collinear variables, working class identification and union household, both remained. Missing data (primarily "not ascertained" cases) were excluded.

3. For the first value, we began by calculating a predicted probability for each individual. This required applying the parameter estimates for each variable to data values for each individual. The predicted probability for a group in a given year is then the average of the predicted probabilities for all group members in that year. Similarly, the incremental difference is the average of the difference, for each group member, between the individual's predicted probability and what the individual's probability would have been without the effect of the group membership.

4. Logit also allows us to identify significant interaction effects among the independent variables. One such effect contrasts with Petrocik's (1981, pp. 84-85) results. Native southern whites of working-class status had a 34% decline in Democratic partisanship, surpassing the 30% decline by southern, non-working-class whites.

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REVIEW ESSAYS

A World of Secrets: The Uses and Limits of Intelligence. By Walter Laqueur. (New York: Basic Books, 1985. Pp. xii + 404. $21.95.)

Regulating U.S. Intelligence Operations: A Study in Definition of the National Interest. By John M. Oseth. (Lexington: University Press of Kentucky, 1985. Pp. xvii + 236. $24.00.)

A Season of Inquiry: The Senate Intelligence Investigation. By Loch K. Johnson. (Lexington: University Press of Kentucky, 1985. Pp. 317. $31.00.)

Secrecy and Democracy: The CIA in Transition. By Stansfield Turner. (Boston: Houghton Mifflin, 1985. Pp. xii + 304. $16.95.)

Intelligence Policy and Process. Edited by Alfred C. Maurer. Marion D. Tunstall, and James M. Keagle. (Boulder, CO: Westview Press, 1985. Pp. viii + 399. $20.00.)

Knowing One's Enemies: Intelligence Assessments Before the Two World Wars. Edited by Ernest R. May. (Princeton: Princeton University Press, 1984. Pp. xiii + 561. $29.50.)

The Missing Dimension: Governments and Intelligence Communities in the Twentieth Century. Edited by Christopher Andrew and David Dilks. (Champaign-Urbana: University of Illinois Press. 1984. Pp. vi ⁵ 300. $27.95.)

Domestic Intelligence and Intelligence and Policy. Vols. 6 and 7 of **Intelligence Requirements for the 1980s.** Edited by Roy Godson. (Lexington, MA: Lexington Books, 1986. Vol. 6; Pp. xiv + 192. $14.95. Vol. 7. Pp. xii + 290. $14.95.)

No theoretical issue is more important and more difficult than the relationship of information and action: important, because knowledge is potential power and politics is about power, difficult, because analysis of the knowledge-action nexus requires understanding of cognition, perception, and communications flow in political systems.

This generation of political scientists has seen, if darkly, the growth of a mammoth intelligence bureaucracy designed to supply policy makers with information and, ideally, foreknowledge about foreign affairs. Yet the relationship of knowledge to action has remained obscure. This is because of intelligence secrecy and the difficulty of measuring, in an elaborate multiorganizational process, the influence of the intelligence product on policy and action.

Recently there has been an outpouring of a variety of publications promising to improve our understanding of the role intelligence plays in foreign policy decisions. This essay focuses primarily on those works that attempt to analyze objectively problems of strategic intelligence policy, organization, influence, and democratic accountability.

Walter Laqueur's *A World of Secrets* is the most significant book since Sherman Kent's *Strategic Intelligence for American World Policy* (Princeton: Princeton University Press, 1949) and Roger Hilsman's *Strategic Intelligence and National Decisions* (Glencoe, IL: The Free Press, 1956). Kent's work was published at the dawn of the age of central intelligence, and is a descriptive and prescriptive analysis of intelligence doctrine that had great influence on the evolution of American intelligence professionalism. Hilsman relied heavily on interviews with producers and consumers, and analyzed the application of intelligence doctrine in the new, post-World War II intelligence system.

Laqueur's book represents a massive team assault on these enduring topics: intelligence doctrine, failure, influence on policy, process, secrecy, and democratic accountability. The book is the product of a research undertaking aided by the Twentieth Century Fund. This permitted the principal author, Laqueur, to organize a group of Washington-based assistants who, one can infer, drafted a number of the chapters. In his preface, the author mentions six drafting coauthors by name. Interviews were held with unnamed Directors of Central Intelligence, Secretaries of State and Defense, and National Security Advisers. The total persons consulted numbered "more than two hundred people in half a dozen countries" (p. xii). Although many of these sources are not specified in the endnotes, the book contains 47 pages of footnote citations. No other work on the subject represents such wide coverage of sources. Here is a work of large-scale "academic intelligence" seeking out the bits and pieces of an ostensibly secret subject and attempting to fit them together in a meaningful mosaic.

After 40 years the central intelligence role has remained obscure. To some, the model is that "intelligence guides policy," in that policy makers precisely informed of external realities can maximize utility in rational choice among policy options. To others, the intelligence apparatus that has evolved since 1947 represents a politicized "invisible government" dominating policy. Laqueur seeks answers to such questions as, What role has intelligence played in foreign policy making? How accurate has the intelligence product been? How well has intelligence been communicated? What are the inherent limits of intelligence estimates? How can intelligence be improved? Are there special problems for secret intelligence agencies in a democratic framework?

In part 1 of the book, Laqueur analyzes the production of intelligence, giving particular attention to economic and scientific intelligence. In this section he describes the intelligence process, and it is replete with gratuitous comments about that process. For example, he writes that "intelligence production, quality and consumer demand varies [sic] enormously according to the political regime under which these take place" (p. 16)—a significant point, but inadequately developed.

Part 2 is the book's most important contribution, essaying an evaluation of intelligence performance since World War II. New insights are offered into the ways in which various presidents viewed and used intelligence, into relationships between presidents and CIA directors, and into the bureaucratic politics of intelligence. However, this part of the book has a gossipy quality and contains some patently inaccurate remarks, such as the assertion that the American "left" (undefined) "opposed the very existence of intelligence" (p. 85). At another point the author says of Vice-President Mondale and David Aaron of the national security staff that their "enthusiasm for intelligence was limited" (pp. 85-86). At another point it is asserted that "intelligence was not considered important, but a source of potential trouble" in the first three years of the Carter Administration (p. 87). Such comments suggest either a bias or that Laqueur's sources were overweighted on the side of the professional intelligence lobby. Nonetheless, the author offers some insightful and useful comments on such important topics as information pathologies within bureaucracies, the compartmentalization of knowledge, the phenomenon of data overload, pressures for conformity, and other organizational factors that shape the quality of intelligence.

Laqueur then turns to concrete examples of intelligence performance. Although no definitive "case study" of any of the events described is presented, the quality of intelligence estimates is reviewed from the earliest days of United States central intelligence. Here Laqueur is able to utilize recently declassified estimates as well as interviews and secondary sources. Special and detailed attention is given to the "Missile Gap" controversy, the Cuban missile crisis, the Vietnam War, and estimates of Soviet missile strength. He uses these cases to demonstrate that intelligence forecasts were sometimes right, sometimes wrong. He also derives from this evidence support for the book's theme: that intelligence estimates "seldom had a decisive influence on the conduct of U.S. foreign policy" (p. 197). In this view, "intelligence can do no more than serve as a useful guide, a compass in a complicated world" (p. 198). Readers may experience some frustration with such ambiguous conclusions. The problem is that Laqueur, like others of us, has difficulty in measuring the influence of intelligence. This may be because many of the secrets are withheld from us long after events have passed.

Part 3 is a somewhat superficial survey of how other democratic nations, specifically Great Britain, West Germany, and Israel, have dealt with the dilemmas of secret intelligence services. The relevance to the book's theme is not clear, because the author concedes at the outset that the United States is different in important respects from the three nations compared. However, a chapter on the Soviet Union focusing on the KGB and the GRU (military intelligence) has substance. Here again, the author observes that the CIA and KGB are not comparable, either in function or size. After describing in outline the organization of Soviet intelligence, the author concludes—without supporting evidence—that "the overall record of Soviet intelligence achievements has been excellent" (p. 243). He suggests that any failures have not been due to ideological blindness, but to lack of knowledge of the outside world, inadequate understanding of foreign political systems, and belief in stereotypes. In this respect, it is not clear how the Soviets differ fundamentally from the West.

Part 4, ambitiously titled "Theories of Intelligence," deals with the causes of intelligence failure and examines the question of whether intelligence is craft or science. He catalogues nicely the various "causes" of failure, such as surprise, bias, ignorance, and deception, but this section is disappointingly lacking in hypotheses, and concludes on the vague suggestion that even though "major mistakes should not often occur" (p. 291), intelligence is ultimately an art rather than a science.

The concluding section, part 5, addresses the future of intelligence. What do all of the organizational history, the bureaucratic gossip, the illustrative examples, and the comparative material add up to? At one point in the book, when he is evaluating intelligence performance in Vietnam, the author accuses the CIA of using "Delphic language" in intelligence estimates. Ironically, *A World of Secrets* has its own oracular quality that leaves the reader uncertain as to the answer to the book's central questions: the impact of intelligence on policy, the causes of intelligence failure, and prospects for intelligence improvements.

On the one hand, one reads that intelligence has "become an essential part of the process in making national security decisions," and "once the policy process is under way, intelligence becomes essential to the government's pool of information," and "no foreign policy official would want to be without intelligence reporting today" (p. 108). On the other hand, the author concludes that "intelligence was never as important in the conduct of policy as is commonly believed, nor is it ever likely to be" (p. 324). Furthermore, intelligence "is an important element in the decision-making process, but only one element" (p. 344), although at the outset of the book, the author states that "no one claims that intelligence has been of major importance in the conduct of the affairs of state" and that "intelligence is not held in very high esteem by those in high places" (p. 4).

Those of us who have studied these questions for many years can sympathize with the difficulty in measuring precisely the relationship of intelligence and policy. With his Delphic pronouncements, Laqueur leaves this fundamental question open. He observes that intelligence is a "prerequisite" for, but "can never be a substitute for policy or strategy, for political wisdom or military power" (p. 338). This begs the question, because we still need to understand how intelligence contributes to such "wisdom," strategy, or the difficult choices regarding military force.

Laqueur's judgments on more specific issues are characterized by less ambiguity. He is skeptical about the value of basic intelligence organization reform, doubts that high technology offers solutions to many analytic challenges, and believes that better performance will result from better recruiting and the use of nongovernmental experts. He concedes a contradiction between free societies and secret intelligence, is wary of separating covert political intervention from the intelligence function, and is ambivalent about the effectiveness of the existing systems of congressional oversight.

One finds no sustained theme or consistent argument, no tested hypotheses, and essentially little encouragement for the development of intelligence theory. One does find an evaluative study of substance that covers most of the important questions that have been raised with regard to intelligence over the past four decades. While the book has its share of obtuse statements and errors, it deserves—primarily because of its impressive coverage of a variety of sources on the subject—to be ranked with the relatively small body of significant descriptive literature in a burgeoning field.

John M. Oseth's *Regulating U.S. Intelligence*

Operations addresses intelligence issues in the context of definitions of national interest. He stresses issues of policy, organization, and accountability, and analyzes the conflicting requirements of intelligence secrecy and public accountability. Unlike Laqueur, Oseth is more concerned with rules, organization, and accountability than with intelligence performance and impact on policy. He focuses on the turbulent period between 1974 and 1984, when the intelligence system was in a crisis of legitimacy and the issue of secrecy democracy was being sharply raised. He is concerned with the important question of how the intelligence function interacts with American political culture, and he explores the conflict between American ideals and the pursuit of external national interests, a subject too long neglected.

Oseth's method is primarily historical. He has combed the published material on the subject with the thoroughness characterizing a first-rate doctoral dissertation. His 39 pages of endnotes and a selected bibliography indicate an exhuastive survey of the relevant literature. He is searching for a better understanding of the substantive points at issue in the intelligence debate over the past decade, of the policy outcomes, and what the debate and outcomes tell us about national self-image and purpose.

This study reviews in detail the Ford, Carter, and Reagan administrations' handling of these issues. He summarizes the debates, among scholars, among intelligence professionals, in the media, and in Congress. The author offers no dogmatic position on the central issues: executive discretion in secret operations, congressional oversight, disclosure vs. secrecy, idealism vs. realism in foreign policy, the Bill of Rights threats to security. Oseth concludes that while intelligence law, organization, and procedures need refinement, new laws will matter less to the satisfactory conciliation of democratic values with security priorities than the "strength and character in our officials" (p. 187). This would seem to equivocate on some of the fundamental issues, but like many observers, he has little faith in new laws and reorganization as solutions.

In sum, Oseth has provided a fine catalogue of the issues surrounding a secret service in a democracy. His book is primarily a descriptive and evaluative account of the intelligence issue over the past 10 years. Anyone intending to pursue a more empirical study of this subject, or wanting a competent synthesis of intelligence events and issues from 1974 to 1984, will find this study required reading.

Loch K. Johnson's *A Season of Inquiry*, is an analysis of the internal politics of the U.S. Senate Select Committee to Study Governmental Operations with Respect to Intelligence. This committee, led by Senator Frank Church of Idaho, was the Senate's response to reevaluations in 1974 that the CIA and FBI and other units of the "intelligence community" had been operating illegally and with little congressional monitoring.

Johnson, a former Congressional Fellow of the American Political Science Association, has written a unique study. His dual concern is intelligence policy and congressional behavior in a time of crisis that cast doubt on the legitimacy of the intelligence system, raised concern about the abuse of presidential power, and produced deep disagreements within Congress about its appropriate role and about procedures with regard to the government's secret intelligence operations.

The author writes from the vantage point of a scholar who has specialized in Congress, as a student of American politics, and as a major participant. He was a staff member of the Church Committee, serving also as the chairman's personal assistant during the sixteen-month Senate investigation. Later he was a staff director of the House Intelligence Committee, which gave him a broader perspective on the congressional role. As a political scientist retrospectively analyzing a process in which he was an actor, Johnson ran the risk of bias. He has overcome this problem, for his book is an objective account of the forces at work in a major Senate investigation of sensitive government policy and operations. Many persons regard the 1975 Senate probe of U.S. intelligence agencies as a circus at best, and a destructive witch hunt at worst. Former President Nixon refers to "the castration of the CIA" and, like others, appears to blame the congressional probes of 1974–1976 for emasculating the intelligence system. Johnson's book should help to dispel this mythology. It offers a balanced view of the "Year of Intelligence" in which both houses of Congress, responding to widespread popular concern, turned a bright and sometimes errant searchlight on intelligence.

Most of the book is a personal memoir, written in the first person, of the stormy life of the Senate Committee. He describes its internal staff politics, its debates over roles and procedures, its conflicts with the CIA and the presidency, and its reflection of the pluralistic nature of American politics. This book brings a realism to a congressional probe rarely offered by authors of legislative case studies. This is the story of how the Senate eventually came to have, a permanent Select Committee on Intelligence, yet failed to agree on a "charter" for the intelligence agencies that spells out the "dos" and "don'ts" for the intelligence system. The book offers a rare insight into the many variables that contribute to congressional investigative behavior.

In *Secrecy and Democracy,* Stansfield Turner approaches the intelligence dilemma from the viewpoint of a professional naval officer, former Rhodes scholar, former head of the Naval War College, and former director of Central Intelligence (1977-1981). He came unexpectedly and reluctantly to head the CIA. He discovered immediately that the intelligence "community" was a highly charged political arena—not a community, but an assortment of competing entities. Indeed, he discovered that even the CIA itself lacked unity and coherence, and in fact was "three separate organizations operating almost with autonomy" (p. 185). He is referring to the three operating branches of the CIA: espionage (and covert political action), scientific and technical development, and research and analysis. In becoming the CIA's director, Turner discovered that it was out of control, at least by his standards of military or corporate command and control.

Turner quickly learned that (*a*) the espionage branch was the elite group with the greatest clout among the three CIA branches, and (*b*) each of the branches operated with a high degree of autonomy and was over-staffed and strongly resistant to centralized direction. The "rogue elephant" had three heads. Turner and his aides attempted, in his four years as director, "to bring the three-headed colossus of the CIA under centralized control" (p. 193). He restructured the organization, hoping to improve personnel, budgets, and plans; he placed his own appointees in key spots; and he strove to improve communications within the agency. Time ran out before he felt a sense of accomplishment, but he reports that when his tenure as director ended in 1981, "I had no fear that the organization was operating behind my back or hiding things from me" (p. 194). He adds, however, that "being confident that the organization was not *out* of control was not the same as feeling that it was adequately *under* control" (p. 194). Turner left the directorship clearly unsatisfied with the agency's personnel procedures and with the outcome of his bureaucratic struggle to control the "three CIAs."

Only in America could, or would, a recent intelligence chief write such a startlingly candid book. Turner had difficulty getting CIA clearance for his manuscript. He was required by the agency to make more than 100 deletions on security grounds. Even so, Turner's book is the most authoritative and frank inside look at the CIA's bureaucratic culture, warts and all, ever published. Discounting his military command approach and the self-justifying aspects of any such memoir, Turner offers observers of intelligence exceptionally revealing testimony. There is insight here also for those interested in public policy, administrative management, and bureaucratic behavior. This candid (Turner admits some mistakes) and critical inside look at the heart of the nation's intelligence apparatus is not reassuring. The causes of many intelligence failures and inefficiencies become apparent. Regrettably, we cannot see the manuscript uncensored by the CIA.

Turner concludes with a catalogue of intelligence reforms that he feels are needed to equip intelligence better for its multiple functions. For example, he supports a revised comprehensive legislative charter for the intelligence system, favors effective congressional oversight, would have a Director of Central Intelligence separate from the CIA head, would combine the CIA's analytic and espionage branches, reduce the emphasis on covert political intervention, and depoliticize the director's role by appointment of a politically neutral person.

I have advocated most of these reforms for many years, and welcome the judgment of a former CIA director. Turner has faith that it is possible to reconcile "the necessary secrecy of intelligence to the democratic processes on which our government is founded" (p. 285). Whether such faith is justified remains a serious question.

Other works published recently add insight into issues of intelligence policy, organization, functions, process, performance, and accountability. *Intelligence Policy and Process,* edited by Alfred C. Maurer, Marion D. Tunstall, and James M. Keagle, is a collection of 20 essays by intelligence specialists, academic as well as practitioners. It probes the many aspects of the relationship of intelligence and policy. The book is the outgrowth of a conference held by the Political Science Department of the U.S. Air Force Academy in June 1984. The collection has been assembled with thoughtful attention to the problems of theory. Its design is coherent, even though its generally sophisticated essays lack thematic unity, as is common in such compendiums. An appended twenty-four-page bibliography and a glossary make the book a useful teaching instrument.

Two volumes of historical essays also deserve attention. Definitive studies of intelligence performance are most likely to be found in historical monographs. In *Knowing One's Enemies,* Ernest R. May has assembled an outstanding collection of case studies, divided into three sections: (1) World War I; (2) World War II in Western Europe; and (3) "Eastern Europe and Beyond" in the Second World War. These 16 monographs analyze what the various participating governments knew and when they knew it, as well as the policy consequences of the quality of the information. In a masterful summary essay entitled "Capabilities and Proclivities," May offers no simplified answers. In essence, the historical record is replete with tragic intelligence failures. What are the lessons implied? They include the following: Organizational change rarely brought intelligence success. Long-range intelligence projections were commonly inaccurate. Policies have failed when governments have attempted to see things from the standpoint of adversary governments. Governments have often identified the wrong enemy. Widely accepted presumptions were "often quite wrong" (p. 540), which is to say that strong biases dangerously pervade governmental intelligence and decision-making structures. More recent experience suggests that most of these lessons remain unlearned.

Andrew and Dilks have assembled, in *The Missing Dimension,* 11 essays that analyze a variety of intelligence-related subjects, with emphasis on the British experience. Topics range from such subjects as "Japanese Intelligence and the Approach of the Russo-Japanese War" to "The Cambridge Comintern." This volume concerns itself with intelligence in foreign policy decisions and also with counterintelligence. This eclectic volume has no dominant theme. Together its essays demonstrate, in the words of the book's editors, "the gradual and erratic professionalization of intelligence communities in the West, and the equally gradual and erratic way in which governments have learned to cope with them" (p. 6). *The Missing Dimension* serves as a valuable supplement to *Knowing One's Enemies.*

Another valuable resource on this subject is the impressive, seven-volume series, *Intelligence Requirements for the 1980s,* edited by Roy Godson, Volumes 6 and 7 have recently been published. Volume 6 is entitled *Domestic Intelligence* and Volume 7, *Intelligence and Policy.* The format of these volumes is that of conference proceedings, with formal papers followed by brief summaries of discussions. The product is a provocative analysis of many aspects of the intelligence system. The primary weakness of these useful collections is that the views expressed are heavily weighted to the right of center on the ideological spectrum, which excludes many significant contrary views. The series is organized by the Washington-based Consortium for the Study of Intelligence.

To read Laqueur is to be better informed about the limits of intelligence and the variety of information pathologies that debilitate decision making. Oseth's work teaches about the American ambivalence regarding secret operations that has produced ambiguous intelligence policy. From Johnson we learn about the inner workings of a Senate committee staff and the prevalent confusion of Congress regarding its role in intelligence accountability. Turner offers us chapter and verse about how an entrenched secret bureaucracy can successfully resist responsible leadership. The many other writers and observers in the volumes cited bring revealing light to the dark corners of government and to intelligence history.

Patently an ample bibliography now can support widespread curricular and research attention to intelligence. Theory remains weak and tentative, and the secrecy of evidence remains a formidable problem if one wishes to go beyond historical and evaluative studies, but absence of information, ideas, and opinions is no longer an excuse to neglect intelligence as a subject for research and teaching.

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Equality and Education: Federal Civil Rights Enforcement in the New York City School System. By Michael A. Rebell and Arthur R. Block. (Princeton: Princeton University Press, 1985. Pp. x + 329. $28.50.)

The Burden of Busing: The Politics of Desegregation in Nashville, Tennessee. By Richard A. Pride and J. David Woodward. (Knoxville: The University of Tennessee Press, 1985. Pp. xii + 286. $24.95.)

A Semblance of Justice: St. Louis School Desegregation and Order in Urban America. By Daniel J. Monti. (Columbia: University of Missouri Press, 1985. Pp. xiv + 207. $24.00.)

Metropolitan Desegregation. Edited by Robert L. Green. (New York: Plenum Press, 1985. Pp. xi + 227. $34.50.)

The authors of these new books on school desegregation differ in their conceptions of equalitarianism. One theoretical approach emphasizes the degree of equality of opportunity that would be afforded if the courts limited their role in desegregation cases to ensuring the removal of all vestiges of segregation mandated by law. This definition implicitly posits that although all students, regardless of race or other characteristics, should have full opportunity to "swim or sink," there is no United States constitutional imperative for racial integration of pupils in classrooms. Also implicit is the need for incentives if this approach is to result in quality education for all.

The second theoretical approach to equality emphasizes structural changes to produce integrated classrooms with equal learning situations, which are considered essential to achieve equality of education. The public choice theory is rejected because freedom of choice is viewed as contributing to school segregation, which must be replaced by a mandatory diaspora of black students via busing, since blacks in black classrooms will not learn as much as they would if placed in integrated classrooms. Standardized test scores generally support this premise, but one must question the validity of these test results because of the failure to factor into the scores the quality of teaching and facilities, pupil transiency resulting in absenteeism and a need to adjust to new surroundings, tardiness, and the degree of parental support or apathy.

It also is an unstated assumption of supporters of this second school of thought that blacks will receive equal opportunity only if they are placed in the same classrooms with whites, because the white ruling majority will not adequately fund black schools. Regardless of whether this assumption is valid, evidence is clear that "white flight," caused by mandatory integration and/or other factors, has resulted in dwindling white support for public schools, which are becoming increasingly black in large cities.

The books under consideration here directly or indirectly raise the question of the adequacy of the courts as Platonic guardians of public schools capable of Solomon-type decisions. *Brown v. Board of Education* reflected the first theoretical approach while hinting at the second, which the Supreme Court subsequently adopted in its decision in *Swann v. Charlotte-Mecklenberg Board of Education*, and then retreated from in *Milliken v. Bradley*.

Attorneys Rebell and Block have written the superior case study among these four books. *Equality and Education* is an insightful examination of the complex issue of school desegregation from the standpoint of political and organizational theory. This book, which focuses upon discrimination in teacher hiring and student services, should be read first, as it provides a comprehensive theoretical underpinning not only for their study, but also for other studies. They maintain that Congress by choice did not define the term *discrimination* in the Civil Rights Act of 1964, thereby ensuring conflict over the meaning of the term, which program administrators and courts would be called upon to define.

Equality and Education is unique among the four books in examining interest groups in school desegregation on other than a "protest" basis. Rebell and Block focus on the Federal Office for Civil Rights' scrutiny of the New York City school system for compliance with the Civil Rights Act of 1964. In the process, they also refer to similar studies done in

Chicago, Philadelphia, and Los Angeles, which they say were part of President Nixon's strategy to win the South and hurt the Democratic machines in these four large cities.

The political power of Albert Shanker, president of the United Federation of Teachers in New York City, is made apparent by the voluntary nature of the settlement between the city and the federal office, which contrasts with the mandatory provisions in the settlements in the other three cities. On the other hand, citizens have limited ability to understand the judicial labyrinth and the subtle issues involved in school desegregation cases, and to hold public officials—including judges—responsible. A major conclusion of the authors is that while administrative agencies have a greater fact-finding capacity than legislatures or the courts, the courts are more effective in monitoring compliance with agreements produced by administrative agencies.

It is somewhat surprising that Rebell and Block did not expand upon their reference to the neighborhood government and related black separation movements of the 1960s. The situation in New York City in the late 1960s was explosive, and the 1969 New York State law establishing community school districts with boards elected by proportional representation (PR) helped to defuse the situation. PR has provided representation in proportion to the turnout of voters by various groups, and has helped to satisfy black demands for control of schools in their neighborhoods.

In *The Burden of Busing,* Pride and Woodard attribute the decision to employ busing to the proposition that there is one dominant culture in the United States, and black children in segregated schools were being excluded from assimilation into this culture. They write: "it remained only for black children to be made into white children with dark skin" (pp. 279-80). In addition, they maintain that black pupils still are stigmatized by lower scores on national achievement tests, and that a number of black leaders are questioning the value of busing. They attribute white flight to a perceived loss of status by whites if their children are forced to attend school with black children.

According to *The Burden of Busing,* educational resources in Nashville could have been distributed equally among all schools without busing, but the latter "was believed necessary for the reallocation of prestige, respect, and deference within the community" (p. 281). The authors attack busing because "it reinforces racial thinking. . . Busing divided the preoccupied parents" (pp. 283-84).

Monti, who in *A Semblance of Justice* remains committed to the ideals of the integrationist movement of the 1960s, views the school desegregation movement in St. Louis as part of reform efforts to assimilate a diverse urban population and maintain order, a view somewhat similar to the one expressed by Pride and Woodard. The desegregation effort, in his opinion, is "a ritual that created only the illusion of change" (p. 182).

It is puzzling that a book could be written on the subject of St. Louis School desegregation without a complete examination of the briefs filed in the various court cases and a detailed examination of the court orders. Only one court decision is given a specific citation and the *St. Louis Post-Dispatch* is relied upon heavily for information. This sociological description and analysis of the school desegregation controversy will disappoint political scientists interested in public law and the political dynamics of such a situation.

Convinced that the desegregation campaign has benefited professional educators by increasing their power, Monti asks: "Why do we leave the administration of reform to the very people responsible for creating or perpetuating 'constitutional wrongs' in the first place?" (p. 185). His solution is radical: courts should dissolve boards of education. The result would be that "suddenly, school administrators would have nothing to shield them from an aroused public" (p. 185). Citizens, however, would be afforded less opportunity to make input into the educational system and to hold it responsible for its actions.

The most telling section of Monti's book is the foreword by Derrick A. Bell, a convert to an antipodal view on busing:

> Even the most committed advocates of integration must now concede what was an all-out court campaign to desegregate the public schools has degenerated into a scatter-gun of still-litigated cases, none of which is likely to alter the status of most black children who continue to receive inferior and inadequate schooling in facilities

> generally as separate in fact as they were once segregated in law. (p. vii)

His solution is to bring "effective schooling to black children in black schools" (p. vii).

Metropolitan Desegregation is the product of a five-year study of desegregation in New Castle County, Delaware, by researchers from the Urban Affairs Programs of Michigan State University. Green sets the tone for the anthology by writing *ad judicium*, "the United States education system prepares many minority group Americans only for menial labor, the military, or unemployment" (p. 3). He is convinced that residential segregation in the northern states is the product of "willful acts of public officials and others operating under official sanction" (p. 9). Green apparently discounts evidence revealing that many minority group members desire to reside in areas where fellow members reside—be they Italian, Irish, Chinese, or black—for reasons of affinity, kinship, and/or support.

Green's bias shows in the adjectives he uses relative to the decisions in the Detroit school desegregation case, *Bradley v. Milliken*. Compare "the district court based its reasoning on sound legal principles" (p. 20) with "the Supreme Court rejected this line of reasoning, relying in particular on its belief" (p. 20). He urges school desegregation on a metropolitan area basis, supports his views on the degree of public opposition to desegregation by specifically citing public opinion polls conducted by a pro-desegregation citizens' commission, and holds the media chiefly responsible for the existing public opposition to desegregation efforts.

The majority of the essays follow Green's lead. Cassandra A. Simons concludes that the grouping of many minority children as "slow learners" "is evidence of the lingering effects of segregated schools' inferior education" (p. 73), but provides no empirical evidence. Meyer Weinberg comments, without supporting evidence, that "desegregation studies of achievement have been on a par with other customary inquiries" (p. 145). John H. Schweitzer, however, wisely writes that "the generalizability of the findings of this study is limited by the unique aspects of the school desegregation situation in New Castle County" (p. 184).

With respect to the role of the judiciary, the conclusion is inescapable that the adversarial system of gathering information employed by the courts unfortunately does not produce accurate and complete information upon which to base judicial orders, since the evidence presented to the court often is incomplete and contradictory. The welfare of the students appears to be of little genuine concern to many plaintiffs. The courts embarked on remedying what they perceived to be wrongs in past segregation laws without a full understanding of the monumental social problems reflected in the low scores of many inner-city pupils on standardized tests. These problems cannot be solved by a judicial busing fiat.

The court-prescribed remedy—busing—to achieving "racial balance" in schools in large cities is impossible to accomplish, because of the sharply increasing black student population and the great mobility of poor families. Busing black pupils from a neighborhood school populated primarily by black students to another school where the black percentage is slightly lower in the name of racial desegregation is not sound public policy. The relatively large cost—money and time of pupils, administrators, and litigants—involved in attempting to achieve "racial balance" in schools through busing raises a question of the cost-effectiveness of this approach to guaranteeing quality education for all pupils. In general, these four books reveal that the problem-solving ability of courts is limited, and that judicial omnipotence in cases involving the Fourteenth Amendment does not ensure the superiority of the wisdom of a United States District Court to that of elected officials. A more flexible approach to improving the quality of education provided by big-city schools—the approach adopted in New York City is a possible example—is essential to ensuring all students an equal opportunity to gain a quality education.

Consolidation of school districts in the St. Louis area and New Castle County further impersonalized the system by increasing the distance between parents and the school board by "light years," probably contributed to "white flight," and resulted in less public support for the school system. In New York City, on the contrary, the unitary school system was opened up to wider citizen participation through the creation of 32 new community school districts with a total of 288 elected school board members, which brought decision making on many educational issues down to the neighborhood level.

What has been the impact of mandatory two-way busing of pupils on race relations? Some evidence suggests that hostility between some whites and blacks has been increased by mandatory busing. This hostility can be manifested most effectively in cities where there is a legal requirement for a referendum for the levying of taxes or issuance of school bonds. New Castle County voters in October 1980, in the first tax referendum since the school consolidation, rejected a school tax proposal. On November 5, 1985, voters in the St. Louis City School District narrowly rejected, for the third consecutive year, a bond issue to renovate decrepit school buildings occupied predominantly by black students. Missouri law requires a two-thirds majority affirmative referendum vote for issuance of bonds. Observers attributed the defeat of the bond proposition to white voter discontent. The United States Supreme Court, in 1971 (*Lance v. Board of Education*, 91 S. Ct. 1889), refused to extend its "one-person, one-vote" dictum to extra-majority referendum requirements. Will the court reexamine its *Lance* decision in the light of the experience in St. Louis and similar experiences that may occur elsewhere?

The increasing disillusionment of a number of black civil rights leaders with school busing may herald the filing of fewer suits in the United States District Court that seek to promote equality of education through mandatory busing, given the reality of "white flight" from the public school system. In addition, the increasing black population of many central cities has resulted in political power passing from white control to black control. As blacks gain control of school systems, they will have less interest in court intervention to promote the ideal of equality, unless such intervention will result in increased financial support for the central city schools.

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Just Health Care. By Norman Daniels. (Cambridge: Cambridge University Press, 1985. Pp. xiii + 245. $32.50, cloth; $9.95, paper.)

With Dignity: The Search for Medicare and Medicaid. By Sheri I. David. (Westport, CT: Greenwood Press, 1985, Pp. xiv + 194. $27.95.)

Political Analysis and American Medical Care: Essays. By Theodore R. Marmor. (Cambridge: Cambridge University Press, 1983. Pp. xix + 268. $12.95, paper.)

These books suggest how the study of health politics may have to change as the problems in health policy change. The challenge today is not so much to realize policy ideals or enact new programs as it is to control an existing health establishment.

Norman Daniels' *Just Health Care* asks what principles should govern the distribution of health services. His standard, following John Rawls, is "fair equality of opportunity." The distribution should guarantee everyone an equal chance for "normal functioning," meaning a person's expected physical potential given his or her age and the conditions of a given society. Daniels' account displays mastery of the relevant literature and is closely reasoned. His deliberateness does make the book somewhat repetitive and technical; those not versed in Rawlsian analytic categories may find it difficult. Daniels applies his scheme to OSHA health regulation as well as to issues of health services.

His most startling conclusion is to question the heavy emphasis in current health policy on institutional care for the aged. Since many elderly would function better if they received care in their homes, he reasons, there should be more home health care and less resort to hospitals or nursing homes. At the same time, because he defines normal functioning relative to age, he thinks too much may be spent on the elderly. The health system gives them more help toward reaching their potential than younger cohorts. Accordingly, some expensive therapies, such as organ transplants, might justly be rationed by age and thus denied to many elderly, as is done in Britain. The argument is bracing but compelling. It amounts to saying that health care should do more to serve life and less simply to postpone death.

Otherwise, the implications of this scheme do not seem radical, at least for American health policy. Like Rawls, Daniels seems to accept the liberal welfare state in essence. He seeks changes mainly of degree. While he denies that the rights of health providers should impede movement toward a more egalitarian health system, he also rejects notions of a "right" to health that some theorists and politicians have used to seek massively greater government control and spending in health. Daniels' approach might justify quite a frugal health system in a poor society.

Nevertheless, he sometimes expresses the moral purism often found in theoretical treatments of policy issues. Political theorists tend by nature to emphasize what would be ideal, rather than political or economic limitations. Daniels says little about how a just health system could be realized politically, and in particular how excessive demands for care could be restrained. Like Rawls, he can in theory set limits to claims on government, but in practice he is loath to reject demands for health protections as long as basic social institutions are not fully just in Rawlsian terms—that is, as long as all inequalities do not serve the least advantaged. If setting limits must await such fundamental changes, political theory can be little help to health policy making today, where cost control is the major challenge.

The other two books say much more about actual politics. The analysis in Sheri I. David's *With Dignity* is largely legislative. The book tells the story of the enactment of Medicare and Medicaid. It carries the reader through every twist and turn of the battle in Congress, from 1958 through to victory in 1965. The book is clearly written and exhaustively researched. The sources include interviews and oral histories as well as public documents. Despite this effort, little about the findings is new. The story is much as in Theodore Marmor's *The Politics of Medicare* and other existing accounts.

David clearly sides with the backers of Medicare against those who opposed it or offered halfway measures. She believes that the popular forces demanding care from outside government held the key to good health policy. That view was most plausible before 1965, when government as yet did little to ensure health care to large groups of citizens. Afterwards, however, the very presence of Medicare and Medicaid helped set off the great health inflation, damaging government budgets and access to care for everyone. Against these problems, the popular politics of health availed little. The coalition that enacted Medicare dissipated, and federal officials struggled with cost control almost alone. Expensive compromises with provider interests were inevitable. David does cover the implementation process, but because of her legislative focus she says little more about it than Marmor did in his book, published 12 years earlier.

The progressive view that the frontier in social policy lies where the people press demands on government has a long tradition in health studies. Ten years ago, Robert Alford contended in *Health Care Politics* that "structure interests" blocked the democratization of health. Not only doctors and other providers but health administrators and reformers, their apparent adversaries, had a vested interest in a pluralist health system in which consumer interests always came last. Another book with a similar viewpoint was Barbara and John Ehrenreich's *The American Health Empire.* More recently, Paul Starr's much-noted study of *The Social Transformation of American Medicine* traced the ascent of the medical profession from impotence in the nineteenth century to a position of cultural authority, wealth, and power over a vast health establishment. There have also been muck-raking attacks on specific health institutions, such as *Unloving Care,* Bruce C. Vladeck's critique of long-term care policy and the nursing home industry.

Such treatments understand the political problem in health as mobilizing a dispersed public against the concentrated interests that operate health services. They say this is tough to do in American politics, because of the pluralism of the system and the influence it accords to well-financed groups. Viewed blow-by-blow, achieving a government role in health was indeed difficult. The battle for Medicare took decades, in part because of provider opposition, and the nation still has no comprehensive health insurance system.

A longer view, however, suggests that government finally does move against overweening interests. Today, federal and state agencies are steadily trimming the prerogatives of doctors and hospitals, just as they previously curbed monopolies, Wall Street, polluting industries, and other damaging expressions of unrestrained capitalism. In particular, the prepaid system of hospital reimbursement that Washington instituted in 1983, called Diagnostic Related Groups (DRG), has sharply checked the upward spiral of Medicare costs. The day may be coming when the providers become, in effect, public servants whose pay and conditions of work are closely regulated by government.

In the new environment, the main political task is no longer to mobilize popular pressures against government. It is to do two quite different things, both even more difficult for American politics: to restrain demands for benefits and to build an effective administrative system for health. Both tasks are necessary to control costs. The history is that middle-class benefit programs like Medicare may be controversial when enacted, but soon become sacrosanct. Politicians then face great pressure to maintain and expand the benefits, whatever the expense. The administrative problem arises because programs are enacted piecemeal, and their implementation is usually neglected. Administrative issues are seldom visible or partisan ones that easily mobilize a broad constituency. American politicians get elected mainly for initiating new programs, not for reforming or managing the old ones. They seldom have the patience for the years of bureaucratic statecraft needed to rationalize health or any other policy area.

Thus, the health analysis we need today is about health politics in the "small," not the "large." It should emphasize administration rather than legislation. It should mainly be about implementation problems in programs that already exist. It should suggest tactics for overcoming the political and administrative barriers to a successful health regime. It should help make American government do what it does not easily do—govern the health system well. We already know how Medicare passed. We need to learn why DRG succeeded. Somehow, the Reagan Administration sold Congress on a tough regime for hospital reimbursement when, only three years before, the hospitals interests defeated a Carter proposal with comparable aims. Was it that Reagan officials presented DRG as an exercise in "incentives" and even "competition," rather than as heavy-handed bureaucratic control? Was it that Republicans can regulate more easily than Democrats because they cannot be seen as anti-business?

Political analysis like this is also an advance over merely economic treatments of health issues. Economists are very good at telling government what it ought to do. For example, Alain Enthoven in *Health Plan* urges that a health-care system based on competition among third-party payers could limit cost increases, while Henry J. Aaron and William B. Schwartz in *The Painful Prescription* show how British-style rationing of care could do so. Each case is compelling in itself, but neither shows convincingly how to institute the reform given the institutions we have. While the authors realize the political barriers, they think that making an impersonal case for reform is the only way to overcome them. That reflects the faith of economics that the core of policy making is optimal choice.

For political analysis, however, the central problem is assembling support for what is optimal. Successful tactics seldom follow directly from the economic case for a policy. That is because public choices are made, not by single officials, but by many, and the interests of each may diverge from that of the collectivity. Any recommendation must be squared with the electoral motivations of individual politicians, and individual officials must be able to carry out the policy within the bureaucracy. Macro policy analysis must be joined to micro analysis of how policy makers operate in existing institutions.

Theodore R. Marmor's *Political Analysis and American Medical Care* is, in large part, a manifesto for this new style of health analysis. The book is a collection of essays Marmor wrote in the 1970s, mostly with coauthors. He contends that political science can help identify what is special about health politics and also contribute to realistic policy options for health. His general discussion of how political science concepts might apply to health is dry, but the idea comes alive in many of the individual essays. The subjects include national health insurance, health planning, Medicaid and Medicare, and various strategies for controlling health costs.

Marmor and his associates examine what each program or policy has achieved or might achieve, given the political constraints and experience to date. They often draw illuminating comparisons between the United States and other countries, particularly Canada. If there is an overall message, it is that authoritative measures to limit health facilities and reimbursement are indispensable to curbing the expansion of the health system. Current American planning and funding policies lack the teeth simply to say *no* to excessive demands. The policy history here and abroad makes the authors doubt that incentives and other market devices will suffice, despite economists' claims, though they can make a contribution. Such "crude measures" as hospital bed limits will likely control spending better than "subtle management measures of theoretical force for which one cannot find large-scale instances of successful use and impact" (p. 173).

The essays suggest the great contribution the new style of health analysis might make. However, some are well-written, some not, and many are too short to analyze their subjects in depth. The discussion of the institutional setting is necessarily limited. They are also pessimistic, perhaps needlessly so. They tend to debunk nostrums in health policy and leave little in their place. Economics proudly calls itself the dismal science, a discipline for making tough choices. In Marmor's hands, political science sometimes seems too tough to offer any solutions at all. However realistic it is, the discipline must remain the platonic master science that expands collective possibilities. If nothing else, the shade of Burke rises up to demand that existing institutions somehow be turned to better account. A longer, more probing kind of study might suggest ways to improve health governance that would be both effective and politic.

Fortunately, a number of studies like this have begun to emerge. Examples include Lawrence D. Brown's *Politics and Health Care Organization*, a study of federal HMO policy; Judith M. Feder's *Medicare*, an analysis of the early rule making in Medicare; and Victor G. Rodwin's *The Health Planning Predicament*, a comparison of health planning in the United States, Britain, France, and Quebec. Another example is Frank J. Thompson's *Health Policy and the Bureaucracy*, a useful synthesis of implementation findings about several major health programs. These studies are based on extended research done mostly inside the health bureaucracy. Each sensitively explains how political and bureaucratic forces have shaped the policies we have. Sometimes the authors make explicit policy suggestions, sometimes not; in any event, the reader or the policymaker learns the parameters of the possible.

The theme of such research, stated explicitly by Marmor and by Thompson, is that health policy making can no longer be as popular as it once was, in either sense of that word. The struggle over Medicare was the greatest progressive battle for health care. Not by accident, it was also the last. The struggles officials now face to limit demands and rationalize the system have only remote political payoffs, not immediate ones. They must operate in a system where heavy political commitments and existing agencies already encumber the ground. If political scientists can help them understand that environment, they have contributed all they can to the daunting tasks of health statesmanship.

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POLITICAL THEORY

The Liberal Future in America: Essays in Renewal. Edited by Philip Abbott and Michael B. Levy. (Westport, CT: Greenwood Press, 1985. Pp. vi + 210. $29.95.)

During the 1960s and 1970s, the various political and philosophical shortcomings of American liberalism were proclaimed in works such as *The Poverty of Liberalism*, *The End of Liberalism*, and *Beyond Liberalism*. In retrospect, the troubles of liberalism in those decades seem minor when compared to its difficulties in the era of Ronald Reagan. Philip Abbott and Michael Levy have compiled an interesting collection of essays on the current dilemmas of American liberalism. The editors are especially concerned with "the difficulty of defining a liberal perspective in a complex world for which liberalism has neither lost its relevance nor proven its adequacy" (p. 11). The essays—all but one of which are published here for the first time—address a wide range of topics. Most of the contributors offer critiques from within the framework of the liberal tradition, while attempting to relate liberal theory to political practice.

The essays fall into two broad categories. Some focus on the broad philosophical premises of liberalism, particularly the relationship between the individual and society, while others emphasize issues of policy and politics. A recurring theme in the former group is the need for liberalism to complement its idea of individualism with a fuller understanding of the social and communal aspects of human experience. In the opening essay, Glenn Tinder examines the liberal conception of liberty. He maintains that the principle of liberty, properly understood, rests on the recognition of an incurable breach between the individual and a society that is always "morally degraded" to some degree. The ideal of liberty, based on this tragic vision, has been distorted by its transformation into the ideology of liberalism. Both laissez-faire and reformist liberals wrongly assume a basic harmony between liberty and other major political values. This argument strikes me as extremely problematic. Tinder seems to assume there is a single, essential ideal of liberty, rather than legitimate competing conceptions. Moreover, he criticizes ideological oversimplification in liberalism without examining the ideas of specific liberal thinkers.

The remaining essays in the first group, although not lacking in points of controversy, are rather more convincing. Philip Abbott observes that while liberalism has made major contributions to historic innovations in the political and economic realms (constitutional government and the market economy), there has been a scarcity of liberal "social inventions." In the contemporary world, where social institutions are increasingly penetrated by political and economic forces, the future agenda of liberalism must involve the creation of new communities with "purpose and commitment," as well as the development of a philosophy that is "truly social." Robert Booth Fowler discusses a relatively neglected topic, the relationship between liberalism and religion. He contends that religion in America serves as a necessary "alternative" to liberalism, as a refuge from individualism and rational skepticism. Neither proponents nor critics of liberalism generally realize the extent to which liberalism needs religion; by diffusing the "costs" of liberalism, religion makes the liberal world more tolerable for many citizens. Jean Bethke Elshtain criticizes contemporary American feminist theory for being captive to the assumptions of liberal individualism. By giving primacy to individual rights and freedom of choice, feminism cannot account for central aspects of social experience. As an alternative to the framework of liberal individualism, Elshtain proposes a feminist theory that incorporates ideals of civic humanism, active citizenship, and participatory politics.

The best of the policy-oriented essays is Michael Levy's critical analysis of the doctrine of the "new property" as a legitimating basis for social welfare entitlements. He concludes that this conception of property rights ultimately undermines the fundamental liberal principle of the distinction between the state and society. As an alternative, he advocates a "neo-Lockean" approach to liberal egalitarianism, emphasizing social and economic policies designed to enhance meaningful opportunity for all individuals to develop their capacities. Alan Stone considers the neo-laissez-faire attack on government regulation. He contends that a key reason for the success of this attack is that liberal defenders of economic regulation have not developed a coherent framework for determining when regulation is appropriate and justified. Stone proposes a framework (essentially derived from mainstream welfare economics) that suggests three main justifications for regulatory intervention: efficiency, the problem of externalities, and considerations of equity. James C. Dick examines the liberal approach to the use of force in foreign policy. He criticizes liberals for focusing on the domestic politics of other nations, an error stemming from the application of liberal individualism to international politics. The liberal perspective tends to treat all individuals as citizens of the world, while it neglects intermediate institutions (i.e., nation-states) and the structure of the international system.

The volume concludes with an essay on liberalism and electoral politics. David B. Hill discusses the decline of the Democratic party and criticizes New Deal liberals for attempting to appeal to voters in ideological terms. He maintains that rebuilding a liberal constituency must begin with self-declared liberals in the electorate, while focusing on a narrow range of issues such as education, health, the environment, and civil liberties. Hill's strategy is based on a very dubious proposition; namely, that New Deal liberalism has been too "ideological." To the contrary, in the current era of conservative ascendency, a conspicuous failure of American liberalism has been its inability to mount a visible ideological counteroffensive. Consider, for example, the conservative attack on the welfare state. While various constituencies have defended certain government programs, liberal Democrats have not offered any comprehensive defense of the welfare state. The political renewal of liberalism is not likely without some sort of ideological renewal. Ideological coherence need not entail adherence to rigid dogma. The reconsideration of liberal policies should not lose sight of the central importance of liberal principles.

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Psychological Perspective on Politics. By Carol Barner-Barry and Robert Rosenwein. (Englewood Cliffs, NJ: Prentice-Hall, Inc., 1985. Pp. iii + 342. $18.95, paper.)

The authors undertake the task of describing "how . . . personal and environmental factors . . . influence political behavior" (p. 8). The task is a difficult one, seeming to require either a synthesis of diverse data and theoretic perspectives into one (or a few) explanatory paradigms, or the use of some other organizing tool to highlight similarities, discrepancies, and inconsistencies. The summary waivers between positivist and psychoanalytic perspectives. While this reflects the diversity of the field, it is discomforting to the reader and often creates difficulties (such as unwarranted causal attributions) for the authors.

The book lacks a central theme. It covers topics as diverse as the philosophy of science, involvement, socialization, leading and following, persuasion, bargaining and negotiation, conflict, decision making, and perceptions of justice. The book is an attempt to define political science and explain all aspects of politics. The authors try to do too much.

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Guilds and Civil Society in European Political Thought From the Twelfth Century to the Present. By Anthony Black (Ithaca, NY: Cornell University Press, 1984 Pp. xv + 280. $32.50.)

This book is a study of the concept of political community as it appears in Western political thought. It is distinctive in that it focuses neither on the geographically local community nor on the idea of the polity itself as a communal entity, but on community in the workplace. As such, it stands as an important intellectual history of the guild.

To have provided such a history would itself be a significant contribution, but Black's achievement is rather more than this. His goal is not simply to understand and relate the history of the guild, but to demonstrate how the guild ideology influenced and contributed to the development of medieval and modern political thought. In this regard, the sweep of the book is quite extraordinary. Black treats the development of the concept of the guild from the Middle Ages to the twentieth century and, in the process, provides substantial treatments of such diverse thinkers as Marsiglio, Althusius, Bodin, Hegel, and Durkheim, among many others.

Throughout, the argument is structured by two sets of oppositions, one theoretical, the other formal. Theoretically, Black pursues the tension in political thought between the guild idea and the concept of civil society. The guild contributes to notions within political consciousness of fraternity and mutual aid, a sense of corporate identity, and the view that collective decisions should emanate from relations of friendship. The concept of civil society, on the other hand, emphasizes principles of personal security and freedom, legal rights, and—increasingly—a bourgeois view of the individual as having claims apart from his particular social connections. It is one of the distinct virtues of this study to have traced in substantial detail the interplay of these notions as they manifest themselves in political thought. The excellent treatment of Marsiglio in this respect (especially pp. 93–94) is merely an example of the way in which Black's conceptual apparatus elicits new and quite persuasive treatments of already well-worked material.

Formally, the book is structured by the tension and interaction between the ordinary consciousness of the guild experience, on the one hand, and the historically conditioned but nonetheless quite distinctive claims of philosophy, on the other. Thus, Black presents evidence not simply about the great thinkers, but about the mentality of medieval townsmen, the political ethos of German guilds during the Reformation, the consciousness of the modern trade unions, and the like. The scholarship here is exemplary, and the presentation is such as to require that philosophical formulations be considered in the light of historical practice and ethos, and vice versa.

In this connection, Black has appended to his study a useful and self-reflective "Note on Method" in which he acknowledges his debt to, yet carefully distances himself from, the work of Pocock and Skinner. He is wary of the notion of "paradigm" in intellectual history, worrying that it presupposes a kind of coherence where there is none. He recognizes the importance of seeing political thought in its political contexts, but wishes also to account for its socioeconomic contexts. Also, he refuses to prejudge the question of whether political thought merely reflects, or in fact has an autonomous impact upon, more "material" processes.

These various methodological principles are very well reflected in the study itself. Indeed, from the persepctive of our own guild, the guild of political philosophers, this book is something of a model of good craftsmanship. It is, however, to be admired on other than purely aesthetic grounds, for I believe that Black has made here a substantial contribution for the history of ideas.

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Revolution: A History of the Idea. Edited by David Close and Carl Bridge. (Totowa, NJ: Barnes and Noble Books, 1985. Pp. 221. $23.50.)

This book is at once a textbook and a work of scholarship. Produced by the disciplines of history and politics at Flinders University of South Australia, it brings together essential components of the study of revolution: the history and analysis of the concept, and several illuminating case studies. By the editors' own design, the book is not comprehensive. Its case studies are all derived from the twentieth century, and the subjects of the various essays were determined by the needs of the students and the interests of the authors.

The result of this design is a fine book that will serve both those who teach the concept of revolution and those who wish to learn something about it. Nearly two-thirds of the book consists of seven essays on the historical development and analysis of the meaning of revolution. The last third is occupied by five case studies: the Soviet Union, China, Nazi Germany, Guinea-Bissau, and France in May 1968. These serve as a valuable addition to a book seeking a student audience. The cases develop the notion of revolution as failed (USSR), as developmental (China), as unworthy (Nazi Germany), as successful (Guinea-Bissau), and as transformed (France, 1968). As such, this section takes the readers through the twentieth century concept of revolution, leads them beyond the European understanding of a revolution, and concludes with the question of whether revolution as a concept makes much sense. It is this section that will serve the broadest pedagogical needs.

The heart of the book, however, is its theoretical development of an idea. This of course is not something new for students of history or politics, but what recommends this particular treatment is its designed contentiousness. It is a wholly contentious book, and delightfully so. Not that the authors contend with one another in print, although the editors admit to a lack of agreement over the definition of the book's central idea. Rather, the book is contentious in the finest sense, because its central idea is contentious. Those who want to learn about revolution rather than imbibe aged academic ideologies of revolution will benefit from reading this book. Those who teach about revolution with some measure of openness will want to read this book also. It challenges as it informs.

Chapters 2, 3, and 4, all by Peter A. Howell, constitute the core of the book's analysis. They are also the most fun. From the ancient Greek polity through European liberalism of the nineteenth century, the common concept underlying revolution is that which underpins all politics until academic political science—the concept of social justice. Here is the effervescence that explodes, alters, and provides the medium for new orders. In his analysis of the history of the idea, Howell contends, paraphrasing Thucydides, that in its oldest form, revolution was thought to lead to a reversal of values:

> Recklessness . . . mistaken for course; prudent delay and any attempt to understand a question from all sides . . . ridiculed as cowardice; moderation . . . regarded as the disguise of unmanly weakness. Fanaticism and violence . . . held to be the marks of a real man, and the seal of good faith . . . neither law nor justice but fellowship in crime. (p. 17)

Students of revolution who take the issues of social and individual justice to heart will recognize the sobering insight of Thucydides, and be perplexed when they recall that in spite of all we know about political life and political violence, there is still something about revolution that makes us sing. What is it that sobers as it makes us sing? Perhaps it is the *ideal* that human beings are capable of justice in the face of its paradoxes. Howell does an admirable job of tracing how the idea of revolution came to be part of the optimism of modernity.

Like all good books, this one should not have its story revealed in a review, but apropos the issue of contentiousness, those pursuing intellectual connections will want to consider the book's fine treatment of the medieval and modern ideas of politics and violence. Imagine St. Thomas Aquinas as a revolutionary and Marsilio of Padua as a reactionary! Readers of all persuasions will find something here. It is intellectual history at its best.

Underlying this history are the value-based issues which make the modern mind uneasy. Yet this is exactly what the study of revolution ought to do. If it is true that revolution is part of modern optimism, it is still true that Thucydides is right. At the root of Howell's essays and of those on Burke, Marx and Lenin, and contemporary theorists of "modernization" is the implicit conception that without the idea that persons count for something and have rights and powers, the idea of revolution itself does not signify. Revolution is merely violent politics. Yet we know that this is not all; still we sing.

R. W. Carstens

Ohio Dominican College

Controlling Nuclear Weapons: Democracy Versus Guardianship. By Robert A. Dahl. (Syracuse, NY: Syracuse University Press, 1985, Pp. xii + 113. $14.95, cloth; $8.95, paper.)

Can democratic governments make wise and informed judgments about matters of exceptional complexity, such as the control of nuclear weapons? Yes, says Robert Dahl, but only if they undertake unusual steps—ones that have been made possible by recent advances in telecommunications—to increase the competence of citizens.

First delivered as the Frank W. Abrams lectures at Syracuse University in 1984, *Controlling Nuclear Weapons* is more accurately described by its subtitle, *Democracy Versus Guardianship.* In the United States today, Dahl argues, decisions about nuclear weapons are made by a technical elite to which we have not so much delegated authority as alienated it—in no meaningful sense do the citizenry exercise control over this elite. If these decisions require technical competence beyond the reach of ordinary citizens, perhaps we need a new governing ideal: guardianship, not democracy.

By *guardianship,* Dahl means a principled authoritarianism: rule by "a well-qualified minority," "governing in the interests of all, fully respecting the principle of equal consideration," and ruling with "the consent of all" (p. 31). Plato's *Republic* is his principal theoretical depiction, the Republic of Venice his closest long-standing historical approximation.

Competence to rule involves two components: technical or instrumental knowledge and moral competence. Moral competence, in turn, involves both an understanding of moral issues and a disposition to act on this understanding (virtue). With issues like nuclear weapons, Dahl argues, technical and moral competence are inextricably tangled—instrumental judgments in these areas require assessments of uncertainty, choices of propensity toward risk, and tradeoffs among values. All of these have inescapably moral dimensions.

Recognition of the interdependence of moral and technical judgments, however, undermines the case for guardianship as well as democracy. Our de facto nuclear guardians may have technical but not moral competence. The historical record of authoritarian regimes shows that their rulers have difficulty in gathering undistorted information and in finding and training suitable successors. Their moral inadequacies are particularly conspicuous.

Who then shall rule? There is no solution to this dilemma—and thus no rescuing of democracy—unless the citizenry can become more technically competent. Rejecting increased citizen participation and better education (conventionally understood), Dahl puts forward a solution he admits is "quasi-utopian." Using interactive telecommunications technology, information on major issues should be made readily available to all citizens. Self-perpetuating panels of experts (like the National Academy of Sciences or major professional associations) would be responsible for preparing the information in a fashion that was both accurate and "appropriate in level and form" (p. 79) to the diverse capabilities of citizens. Because no citizen could expect to become informed about all the difficult issues we face, a *minipopulus* of about 1000 should be chosen, by lot, to constitute a "highly informed body of public opinion that (except for being highly informed) is representative of the entire citizen body" (p. 76). Decisions of each minipopulus would not be binding. We would still have Congress, a president and the Court. Rather, the minipopulus would "stand for" the public, "reflecting public opinion at a higher level of competence" (p. 88).

These innovations are put forward only tentatively, and Dahl says little in response to several immediate objections that arise. Could the panels of experts escape politicization? Such associations "are probably about as independent of external controls as any organizations in our society" (p. 84), Dahl asserts, but there has been little reason in the past to try to control or co-opt them. Could the information they provide be neutral? Dahl seems to think it could, but doesn't this fly in the face of the interdependence of the instrumental and the moral? And what would become of representation by and the accountability of elected officials under such a scheme? How would we expect the citizenry as a whole to assess divergences between the judgments of a minipopulus and those of elected officials?

Dahl suggests that "small scale experiments should precede large scale adoption" (p. 89). His stress is not so much on the solutions as on the clarification of "a problem of considerable urgency" (p. 89). In recent years, Robert Dahl has challenged us to see what a commitment to democracy may require of us today, particularly an extension of popular control to the workplace and the economy. With *Controlling Nuclear Weapons* he addresses a different modern difficulty. This book cannot be said to add much that is new to democratic theory, but it does bring a vital issue into clearer focus. It is a readable reaffirmation of democracy in the face of the most troubling moral issue of our time.

DOUGLAS C. BENNETT

Temple University

Distributive Justice: A Social-Psychological Perspective. By Morton Deutsch. (New Haven: Yale University Press, 1985. Pp. vii + 313. $27.50.)

With some willing suspension of disbelief I am prepared to accept that the social psychological laboratory models important aspects of the social world we all live in. Do university students engaged in card-sorting tasks share something with workers sorting things on an assembly line? When they play a rubbish removal game under circumstances of cooperation or competition do they reveal processes analogous to those of small business men whose trucks remove real rubbish on real streets? Certainly these tasks reveal as much about psychological processes as the eternal prisoner's dilemma games—probably more. Since the findings of these laboratory studies are substantial improvements over the philosopher's intuition about human behavior and the economist's often naive reliance on rational maximization, the suspension of disbelief is willing, but temporary, for institutions are not laboratories and games are not the same as careers. It is the psychologists themselves, at last, who warn of the situational specificity of response patterns.

A little over ten years ago Morton Deutsch asked: "Equity, Equality, and Need: Which Value Will be Used as the Basis for Distributive Justice?" Here he reports studies that contribute answers to this question, and that correct his earlier answers—in an ingenious array of experiments, some of them reported here for the first time. Deutsch and his coworkers set subjects (mostly university students) to work decoding poetry, sorting cards, estimating jelly beans, improvising parts in plays, playing business games, and reporting throughout on their motives and feelings. Some tasks are cooperatively structured, some competitively structured, and some permit each subject to go his or her own way individualistically. The concern with justice is addressed by varying the reward system: to each according to his score or contribution, to each equally, according to need, or winner-take-all. The findings are fascinating.

Earlier Deutsch suggested that the justice of "equity" (the psychologists' term for rewards proportional to the contribution to some enterprise) would be favored where productivity was highly valued. In the experiments Deutsch finds that "there is no reliable or consistent evidence to indicate that people work more productively as individuals or as group members when they are expecting to be rewarded in proportion to their performances than when they are expecting to be rewarded equally or on the basis of need" (p. 198). Is the capitalist wage system (and, in practice, that of command economies too) based on an illusion? Deutsch notes that this finding applies only to tasks which have some intrinsic interest to subjects who are motivated to do well on other grounds; it would not, he says, apply to alienated labor. (He does not report that research on the effects of extrinsic rewards for tasks with intrinsic interest have usually found working-class subjects responding differently from middle-class subjects.)

Where the rewards are offered for performance on group tasks requiring cooperation, competitive reward systems—such as those implied by scoring each individual for his contribution to the group score—impede group performance, for they inhibit sharing of information or exchanging cards or pieces of a puzzle. Because equal rewards to group members avoids this difficulty, equality under these circumstances is a form of justice with higher productivity. In line with many other studies, Deutsch finds that, in contrast to proportionality, the justice of equality promotes greater group harmony and more favorable attitudes toward fellow workers. This confirms his earlier hypothesis.

Before the experiment, most subjects preferred reward systems proportional to individual scores or to individual contributions. On the other hand, people tended to increase their preferences for whatever justice system they worked under. If proportionality is the justice of the market economy, it fits people's preferences—and reinforces them.

The findings in the four chapters dealing with these experiments are rich and often counter-intuitive. They deal with awakening the sense of injustice (social support greatly helps people's generally obtuse perceptions of unfairness to others—and to themselves!). People prefer rewards based on achievement instead of effort (effort-based rewards lead to work simulation and suggest shirking; they are disliked). There are some interesting personality correlates of justice preferences. The various distribution systems change attitudes in a particular way: people tend to accept and generalize the premises of the systems they work under, etc.

But the book is enlarged by essays on cooperation and conflict resolution, interdependence, worker self-management systems and preventing World War III—often with a genuflexion in the direction of the altar of justice but without serious worship. As a consequence, the empirical justice studies—which carry, I think, the main freight of novel material—are condensed to the point where one must struggle to distinguish the variables and to extract their importance. The struggle is worth it. The ingenious and significant research reported here bears critically on the questions raised by philosophers like John Rawls and Ronald Dworkin, political philosophers like David Miller and William Galston, and economists like Amartya Sen and Lester Thurow.

ROBERT E. LANE

Yale University

Nationalism, Ethnocentrism and Personality. By H. D. Forbes. (Chicago: University of Chicago Press, 1985. Pp. x + 219. $27.50.)

Since the writing of *The Authoritarian Personality* in 1950, a number of attempts have been made to extend the principal themes of that classic work in a way that advances political philosophy. Forbes's work is another example of such a piece. Though the recognition of the theoretical potential of *The Authoritarian Personality (AP)* is laudable, Forbes unfortunately fails to add much of substance to its theoretical thrust.

The reasons for Forbes's failure are predominantly two-fold. The first is that Forbes is wrestling with a massive collection of dissertation data (collected in 1968) that unfortunately does not tap any significant theoretical vein. The second is Forbes's own confusion concerning the theoretical state of the *AP* argument, as well as his confusion over why critical theory, his attempted avenue to theoretical advancement, is itself mired within the ideological debate over the role of psychology within political theory.

With regard to the empirical difficulties of the work, Forbes's two hypotheses are:

> 1. Extreme nationalists are recruited disproportionately from those with the authoritarian structure of personality; and
> 2. Different nationalists' attitudes are structured, or interrelated, in such a way as to justify using the term "ethnocentrism" to describe the attitudes of extreme nationalists. (p. 3)

His findings, the result of questionnaires administered to French-speaking and English-speaking Canadian high school students, is that there are different kinds of nationalism. Specifically, what Forbes finds is that ethnocentrism is only found to correlate with nationalism in those circumstances in which the outgroup is clearly different from the ingroup. At the least, such a finding is already implicit within *AP*; at most, it still does little to advance political philosophy.

The attempt to advance political philosophy beyond the findings of Forbes's study is even more frustrating. Forbes, to his credit, does understand that *AP* is a clear outgrowth of the Frankfurt school of thought that we know as critical theory. He is aware that works such as *AP* are the result of a historically rooted admixture of Marx and Freud, and that the writings of Horkheimer, Fromm, Reich, and, of course, Adorno himself all attempted to deal with the psychological or, as it is frequently called, the "early" period of Marx. Yet if Forbes gives such theoretical significance to the critical perspective, one must ask why the almost paltry scope of the two hypotheses of the book is not supplanted by more than an occasional reference to critical theory. Within 193 pages of text, critical theory is abandoned after page 15 until it is resurrected on pages 145–148, and later on pages 189–193 (with the ironic disclaimer that "noone familiar with critical theory would lightheartedly undertake to plumb its depths and report the results in a page").

Even there, the "doctrine (or method) of liberation" that Forbes finds in critical theory is searching for a kind of "objectivity" that in turn is the result of internal contradictions between "objects" and "the claims made on their behalf" (p. 191). The difficulty with such a theory of liberation is that it deals with only the contradiction between reality and objective, between promise and performance. What *AP* offers, which Forbes seems to have little sense of but which others have dealt with quite precisely (see, Christian Bay, *The Structure of Freedom*, New York; Athenium, 1968) is that *perspectives* upon reality differ widely between the authoritarian and the anti-authoritarian personality. Stated another way, the latter-day questions surrounding *AP* have dealt with relative subjectivities, not objectivity.

There is now more than adequate evidence that there are significant and testable *distinctions* between psychologies that are highly relevant to differentiation between the political left and the political right. Such distinctions are available not only within the work of political scientists like Bay, but also within the work of cognitive psychologists like Herman Witkin, Joseph Royce, and Howard Gardner.

To resurrect a classic work requires that one be aware of what has gone on since its demise. To attempt a theoretical advance upon a significant work without an exploration into the difficulties and biases of that work's theoretical framework, particularly within an area so ideological in its origins and implications as the authoritarian literature is, assures that the current work will be but a small contribution.

WILLIAM P. KREML

University of South Carolina

Georges Sorel: The Character and Development of His Thought. By J. R. Jennings. (New York: St. Martin's Press, 1985. Pp. xi + 209. $25.00.)

J. R. Jennings's study manages the difficult task of combing intellectual biography and the analysis of ideas. It is especially good at making sense of the transitions that are notoriously a feature of Sorel's thinking. The book is compact and clear, the proportions of its discussion judicious, and its scholarship faultless. It is one of rather few works on Sorel that one could recommend to students without fear of their being confused or misled: At the same time, it is a sophisticated work (but unpretentiously so) that one ought carefully to consult in doing research on any of the diverse topics it discusses. It is one of several recent pieces of evidence that Sorel scholarship is at last being given the imaginative treatment it deserves.

One French commentator (quoted by Jennings) has said that Sorel's writing "excluded order, clarity, and, in general, all that would make the assimilation of his ideas easier." Jennings himself elaborates on the difficulty of reading Sorel's work, referring to its sheer extent and diversity, its frequent abstruseness, its heterogeneity of level, and the volatility of the hopes (and the despair) that impelled it. He adds that "pluralism" precluded Sorel from offering any unified or systematic exposition of his thinking (pp. 176–77). Pluralism is not inherently unsystematic. If Sorel had held that a single set of philosophical principles dictated identical conclusions in diverse fields—Jennings speaks of "methodological, scientific, epistemological and ethical pluralism" (pp. 12, 15)—then he would have been a systematizer indeed. However, Jennings does not take Sorel's pluralism to be anything so elaborate or developed. It is something more like a disposition than a theory, and must be defined in a way supple enough to embrace several potentially separable things: conflicts between disciplines, conceptual diversity within disciplines, differences between types of explanation, incommensurability, as well as a sheer stubborn sense of the complexity of life and a hatred of the naive.

What is called pluralism here might possibly invite, the alternative label of *dualism.* Dualism may of course be merely an impoverished pluralism (or a monism *manquè*) but it need not be, and there seems to be a difference between positing an indefinite number of possible points of view and insisting on exhaustive choices. Very often, Sorel does the latter. He speaks of the artificial and the natural, the inside and the outside, the abstract and the concrete, the psychological and the scientific, and so on. Even his pluralistic doctrine of communities (*cités*) of enquirers tends, effectively, to collapse, into a "polar" opposition of science and religion (p. 11). It would be enormously interesting to examine the relation between Sorel's disposition to pluralize, or to open questions to an indefinite number of answers, and his disposition to dualize, or to insist that answers of only two kinds can count. It is among the many merits of Jennings's book that it poses such constructive and far-reaching questions.

RICHARD VERNON

University of Western Ontario

The Ivory Tower: Essays in Philosophy and Public Policy. By Anthony Kenny. (New York and Oxford: Basil Blackwell, 1985. Pp. 137. $29.95, cloth.)

Kenny's collection of philosophical essays is intended to show that the concepts of intention, voluntariness, and purpose play a crucial role in legal and political issues of broad and general interest. The nine essays in this volume fall into two categories: philosophy and law, and philosophy and war. An epilogue consists of one essay on academic freedom.

Part 1, on the philosophy of law, contains four papers. The first two, "Direct and Oblique Intention and Malice Aforethought"

(1976) and "Intention and *Mens Rea* in Murder" (1977), seek a definition of murder which adequately takes into account the role of intention. Both essays present fundamentally the same arguments. Kenny uses the 1974 case of *R. v. Hyam* as a case study. The case concerned a woman who, intending merely to frighten away a rival for her lover's affections, set fire to the rival's home thereby resulting in the death of the rival's two children. The woman was charged with murder in the death of the children and subsequently convicted. While agreeing to dismiss the appeal, the Court of Appeals' majority judges in the case cited different views of intention required for murder. Can a single definition of murder be given? Kenny appeals to Bentham's distinction between direct and oblique intention and the position of Lord Hailsham, one of the majority judges. The result is that murder is "an act which causes death with the intent either to kill or to create a serious risk of death: the intent in each case is to be direct. The intent to kill should be taken to include the (direct) intent to bring about a state of affairs from which one knows death will certainly follow" (pp. 14, 29).

"Duress *per Minas* as a Defence to Crime" (1979) is Kenny's reply to Lord Kilbrandon's paper on duress delivered at a Royal Institute of Philosophy conference on the philosophy of law. The question is: "Should duress provide a defence or mitigation to a charge of murder?" (p. 32). The majority held, in the 1975 case of *Lynch*, a victim of IRA threats, that duress was indeed a legitimate defence. Kenny proceeds by laying out two main arguments permitting duress as a defence and one argument prohibiting such a defence. In the end, Kenny, invoking the opinion of Lord Kilbrandon (who was in the minority in *Lynch*) and the view of English legal historian J. F. Stephen that "criminal law is itself a system of compulsion" (p. 37), wishes to disallow duress as a defence for murder. "It is a very great misfortune to be placed in a situation where one must kill and suffer the consequences, or be killed oneself: any man must pray never to be thus placed between the devil and the deep blue sea. But if the law takes away the deep blue sea, a man will go wherever the devil drives" (p. 38).

"The Expert in Court" (1982), Kenny's Blackstone Lecture of 1982 and the longest of the papers in this volume, focuses on four weaknesses in the law regarding expert evidence: (1) present law on expert evidence allows experts to adopt in various ways the functions of judge, jury, and legislature; (2) present law on expert evidence invites damage to the professions of the experts; (3) while the concept of genuine science is a necessary condition for the criterion for expert evidence, courtrooms are not appropriate places for distinguishing between genuine and pseudosciences—Kenny does provide a criterion for what constitutes a science; and (4) present legal procedure concerning expert evidence is flawed in two ways: an adversary legal system is often inhospitable to the proper use of experts and "there are no experts on morality" (p. 61). Kenny offers three broad corrective measures ranging from the establishment of registers of genuine scientific disciplines to empowering the court rather than the litigating parties to seek expert evidence.

Part 2 contains four similar essays concerning the morality of possessing nuclear weapons as a deterrent to war. In its broad outline, the composite argument of these four essays goes as follows:

(1) The destructive use of nuclear weapons in the conduct of war is immoral because it would unavoidably involve the intentional killing of innocents (this, for Roman Catholics would, apart from any utilitarian moral grounds, violate an absolute divine prohibition), and it would violate the Socratic moral dictum that "it is better to undergo wrong than to do wrong," and "the evil that we would do, if we used nuclear weapons in a major war, would be incomparably greater than the evil we would suffer if the worst came to worst after (unilateral) nuclear disarmament" (p. 116).

(2) The maintenance of nuclear weapons as a deterrent to war is immoral because it is extravagant, it is dangerous, and most important, it is murderous. It involves a willingness or intention to commit murder on a grand scale and "one may not intend even conditionally to do what is forbidden absolutely" (p. 72). (This argument appears first in "Counterforce and Countervalue" [1962] and has been seriously challenged [see Gregory S. Kavka's "Some Paradoxes of Deterrence," *Journal of Philosophy*, v. LXXV, no. 6, June 1978, and Kavka's "Nuclear Deterrence: Some Moral Perplexities" in Douglas MacLean, ed., *The Security Gamble: Deterrence Dilemmas in the*

Nuclear Age, 1984]. One wonders whether Kenny's unelaborated remark that this essay "contains some Catholic assumptions which I no longer share" [p. 2], was affected by such challenges since his subsequent restatement of this key argument in "Better Dead Than Red" [1984] and "Logic and Ethics of Nuclear Deterrence" [1984] is somewhat weaker.)

(3) It therefore follows from (1) and (2) that the NATO nations should pursue unilateral nuclear disarmament on moral grounds. While unilateral disarmament by the West would leave it vulnerable to Soviet nuclear blackmail, the risk is preferable to that of nuclear war. "The prospect of standing defenceless before Communist Russia is indeed a sombre one. But that does not justify us in covenanting with the NATO powers to commit murder" (p. 74).

The final essay of the volume, "Enemies of Academic Freedom" (1984), takes a somewhat conservative approach to external threats to academic freedom (e.g., governments) and internal threats to academic freedom (e.g., from its abuse by professors and students).

Anthony Kenny's collection of essays is at once stimulating and redundant. "The Expert in Court" is imaginative and productive. Part 2 is unduly duplicative: "The Logic and Ethics of Nuclear Deterrence" would suffice. Four of the essays have been published previously.

B. G. Hurdle

Virginia State University

In the Name of Eugenics: Genetics and the Uses of Human Heredity. By Daniel J. Kevles. (New York: Alfred A. Knopf, 1985. Pp. x + 426. $22.95.)

Writing with commendable ease and clarity of style, Daniel J. Kevles, professor of history at the California Institute of Technology, has crafted a painstaking investigation into the dynamics of eugenics ideology as it has evolved over the past century in the United States and Great Britain. Kevles's essential strategy is to present eugenics advocacy through the socialization, career patterns, and writings of its principal protagonists. We are introduced to and become intimately engaged in the lives of Francis Galton, Karl Pearson, Ronald Fisher, Lionel Penrose, and Julian Huxley, among many others. Their private papers provide a fine repository of data, while accounts obtained from various close kin and professional associates enable the author to report further penetrating insights. The mix between "liberal" and "conservative" political strains of commitment to the cause emerges with incisiveness, and Kevles well describes the naivete and bias which often have colored both orientations.

A critical question is the extent of the relevance of this study for political science inquiry. That issue becomes prominent because the author, in his final 50 pages, makes bold to analyze the cultural implications of sociobiology, recombinant DNA, abortion, I.Q. testing, and human genetic engineering taken together as constituting a "new eugenics" capable of exerting powerful (and, at root, deleterious) influence upon contemporary and future public policy considerations. Kevles's treatment of these materials is not entirely satisfactory. For one thing, his skills as a historian are somewhat inappropriate to this portion of his task. Second, he has not given himself enough time to develop a balanced and in-depth perspective. Part of the problem here is that he has allocated five chapters (12–16) to the growth of human genetic science, a discussion that is not directly apposite either to the eugenics he addressed earlier or to the new eugenics he plans to address. Third, his modus operandi lacks methodological rigor. He conducts personal interviews with E. O. Wilson and Richard Lewontin, but not with Arthur Jensen and Jonathan Beckwith. We are never told the criteria by which respondents were selected and why. Fourth, and most important, there are serious gaps in the presentation. Wilson's *Sociobiology: The New Synthesis* is probed with objectivity and understanding, but nowhere can a mention of the more salient *Genes, Mind, and Culture* be found. Cloning is disparaged, but Jeremy Rifkin's machinations receive not a citation. Park Gerald is interviewed, but the fact that some of his colleagues succeeded in putting a damper on his XYY chromosome research fails to attain footnote status. The individual's freedom to learn about his or her own genetic makeup is considered, but the scientist's freedom to perform genetic experimentation within constitutionally acceptable parameters is never even hinted at. In general, this discussion is too anxious to link the social implications of contemporary breakthroughs in genetics and evolutionary biology with the sophistries of main-line eugenics, thus bearing witness to the author's normative predilections.

All in all, Kevles has rendered a distinctively valuable contribution with his penetrating historical review. What we now need to complete the picture is a sequal entitled *In the Name of Euculture,* which would document with equal vigor and scholarship the careers and preachments of those who today and yesterday have argued that genetics counts for nothing in explaining hominid social and intellectual life, that our species therefore possesses cultural choices constrained only by environmental circumstance, and that contrary propositions should not be entertained even as hypotheses to be studied scientifically, else man will be tempted to deracinate his unique moral integrity.

IRA H. CARMEN

University of Illinois

Ideology and Soviet Industrialization. By Timothy W. Luke. (Westport, CT: Greenwood Press, 1984. Pp. xi + 283 $35.00.)

Luke's goal in this broad-ranging examination of the modernization of Russia is to offer a "culturalist explanation of Soviet industrialization." Such analysis entails both an examination of the role of the intelligentsia as the carriers of the values of modernization and industrialization, and of the place of Bolshevism as a "civil religion" that informed and gave impetus to social and economic change in prerevolutionary and Soviet Russia.

In his analysis of the revolutionary Russian intelligentsia, Luke argues that such activist social elements substituted themselves for the weak bourgeoisie that had failed to develop spontaneously within Tsarist society. Because of their liberating role in shattering the social and economic complacency of society and their déclassé identity as a moral community defined by a shared revolutionary consciousness and a vision of modern secular society, the intelligentsia performed the same function as the Calvinist thinkers during the Protestant Reformation in generating a "civil religion" built upon a messianic vision of society and a mutually supporting array of moral and organizational imperatives that gave institutional form to a particular brand of political activism.

For the working class, in whose name the revolution was made, the new civil religion translated both into concrete manifestations of worker activism and discipline and into more general forms of expanded cultural awareness that linked literary and artistic movements with the regime's attempts to create a culture of modernization combining moral and aesthetic dimensions with the formation of a work ethic stressing discipline and order. The Stalinist revolution of the 1930s altered the gradualist approach to economic and cultural revolution based upon the themes of worker education and participation, and substituted what Luke terms the "statist" approach to industrialization based upon an intensification of the class struggle and the creation of a technologically sophisticated industrial infrastructure. What had been a broadly defined reliance on a cultural revolution stressing the interlocking cultural, social, and econmic elements of modernization yielded a single-minded fixation with industrialization and the creation of an elite class of new Soviet workers concerned with the material incentives provided by a regime that fostered their emergence for political as well as economic reasons.

While Luke's study is a competent and readable account of the cultural aspects of modernization and industrialization, it does not break any new ground, in terms either of our understanding of the symbiotic relationship between economic and cultural affairs or of our comprehension of the political motives that led to the shift from the culture of modernization to the culture of industrialization. His descriptions of the disciplinary and incentive program by which the regime sought to reach its goals are perhaps the best contributions of this work, for they do place within a broader political and cultural framework programs that seem at face value to be motivated by short-term expediency.

Another major shortcoming of Luke's work is that he has failed to place the evolution of a workers' culture in a perspective of larger social and cultural developments. While it struggled with the problems of modernization and the development of a culture receptive to industrialization, the regime also had to forge a new comprehension of the role of the intelligentsia per se and of the new emerging middle class, both of which colored its treatment of the working class.

DONALD R. KELLEY

University of Arkansas

At the Dawn of Tyranny: The Origins of Individualism, Political Oppression, and the State. By Eli Sagan. (New York: Alfred A. Knopf, 1985. Pp. xxiii + 420. $22.95.)

Eli Sagan's most recent book explores the darker side of the human spirit and the darker side of politics through an examination of the origins of the state. The public sphere, Sagan notes, did not always exist. In primitive societies, the bonds of kinship determined all human relations. Concepts of law, justice, and politics were defined by highly structured kinship systems: "all politics was family politics" (p. 226). What would drive the people of a primitive society to leave behind the comfortable world of kinship relations, and to put in its place a world of strangers in which nonkinship relations took precedence over the ties of kin? And why, in taking this revolutionary step into the state, would they also invent political tyranny, human sacrifice, social stratification, and economic oppression?

Seeking clues to the destructive forces of modern politics, Sagan investigates the stage of human society that first invented the public sphere. He locates what he calls complex society in the ancient kingdoms of Buganda, Tahiti, and Hawaii, which were in a transitional stage of development when westerners first made contact with them in the eighteenth and nineteenth centuries. Lacking the literacy and complexity of archaic civilizations (such as ancient Mesopotamia, Egypt, and China), they nonetheless displayed far more political and cultural sophistication than the primitive tribe. The creative and energetic inhabitants of complex society gave the world monarchy, aristocracy, bureaucracy, and specialized labor—as well as epic poetry, theatre, and the fairy tale. Paradoxically, these same vibrant and inventive people showed the world a more terrifying facet of the human character. Along with a novel emphasis on imagination and individuality came a new cruelty and aggressiveness: their kings regularly practiced a ritual homicide unknown to primitive society. The "magnificent heroes" who created the first states were at the same time "sadists and pirates and megalomaniacs" who destroyed whatever stood in the way of their quest for omnipotence (p. 318). Politics thus emerged from the kinship system in the garb of tyranny. To Sagan, it is that legacy with which we struggle today, and that legacy which we must understand if we are to limit the potential for atavistic violence in modern life.

At the Dawn of Tyranny gives a rich and detailed portrayal of complex societies and the people who lived in them. Sagan's primary purpose is theoretical, however. The last section of the book provides a psychoanalytic theory of social development that Sagan hopes will explain the relationship between politics and tyranny. In an analysis based largely on the work of Anna Freud and Margaret Mahler, he posits a "universal human impulse toward individuation" as the force that brought about an end to the kinship system and the birth of the state (p. 233). According to this psychoanalytic psychology, human beings experience a powerful inclination to separate themselves from their families (and particularly their mothers). The bonds of kinship, apparently, do not fully satisfy. Separation brings anxiety, though, and a subsequent ambivalence about individuation. Primitive society avoids separation anxiety by repressing individuation; complex society promotes individuation at the price of deep uneasiness. Hence it "preserves a tremendous fear of re-engulfment" (p. 363). Tyrannical kings alleviate that fear by fulfilling a fantasy of omnipotence. So, too, does ritual sacrifice, for the power over life and death is the ultimate power. The great rewards of individuation—the creativity and energy that flow from it—come at a terrible cost in aggression (p. 363). They will continue to do so, Sagan suggests, until the wounds of the separation process have fully healed. Tyranny will exist as long as people yearn for the lost bonds of kinship (p. 294).

Sagan's psychoanalytic perspective persuades but does not wholly convince. Its weakness lies, first, in its failure to explain the exceptional. Given that the Nuer, unlike the

Baganda, retained the kinship system, how do we know that the drive toward individuation is universal? Sagan concerns himself with the impact of the psyche on culture, and in doing so ignores the impact of culture—of political institutions, power relations, economic forces, and social convention—in shaping the psyche. "The psyche," he writes, "is not the given, with all social forms being variables" (p. 347). Sources of change can be found in society too —and especially in society's child-rearing practices. Still, "the energy that drives the whole history of the world is the force of the psyche struggling to fulfill its developmental destiny" (p. 383). In the end, Sagan claims too much, even while his analysis intrigues.

Loose organization and lack of focus also detract from Sagan's work. He introduces his theoretical framework belatedly, and thereby maroons the reader in an ocean of detail. Nevertheless, this is an important book. Too often, students of politics fail to take seriously the impact of kinship relations on the public sphere. Sagan reminds us to look carefully at the legacy of the family. "The state form of politics," he warns, "has never resolved the problem that it owes its origin to the breakdown and transformation of the kinship system" (p. 372). When the bonds of kinship stood in the way of the development of the human spirit, tyranny—and the fantasy of omnipotence it engendered—acted as a liberator. That fantasy now threatens us. Confusing individuality with power, we have promoted a self-seeking individualism at the expense of community. Freedom requires that we relinquish our longing for omnipotence, and that we seek new forms of community based on equality rather than oppression.

LAURA GREYSON

Rollins College

Women in the History of Political Thought: Ancient Greece to Machiavelli. By Arlene Saxonhouse. (New York: Praeger Publishers, 1985. Pp. xii + 210. $39.95, cloth; $12.95, paper.)

This important book raises questions that are central to the study of women and the understanding of political life. It should be read by advanced scholars and students alike. It is both an examination of the position women occupy in the political thought of several classical theorists, and a critique of the failure of liberal political theory to take adequate account of women. Liberal theory, which ignores sexual differentiation in speaking of human equality, views women only as potentially equal members of the human community who have been denied rights given to men. By contrast, says Saxonhouse, preliberal theorists see the female as "a limit on the possibilities of politics," a member of the polity "whose difference from the male must be acknowledged and incorporated" (p. 9). Her project, then, is to recover that understanding of the social and political world which viewed women and the private world as "a part of, rather than just addenda to, political life" (p. vii).

This she does expertly, and in some instances brilliantly. A preliminary chapter contrasting liberal and preliberal views of political life is followed by chapters analyzing Homer and Greek playwrights, Plato, Aristotle, the Romans (Cicero, Seneca, Virgil), Christian thinkers (St. Paul, Augustine, Thomas Aquinas), and Machiavelli. Her interpretations of the *Oresteia*, *Antigone*, and *Trojan Women* are subtle and convincing explorations of the tragedians' understandings of the ineluctable claims of family as well as those of the city. Her reading of women's role in Plato's *Republic* as a foil for explicating the relationship of the philosopher to politics is controversial but deeply intelligent (for an alternative interpretation see Susan Okin, *Women in Western Political Thought*. Princeton: Princeton University Press, 1979). The chapter on Roman thought is a valuable exploration of texts rarely examined for their perspectives on women. Saxonhouse's reflections on early and medieval Christian thinkers reveals that their acknowledgement of women as men's spiritual equals is often accompanied by a denigration of women's traditional reproductive and family activities.

By rescuing the world of women from the oblivion into which it has fallen in most histories of political theory, Saxonhouse has performed a valuable service. Writing of Virgil's *Aeneid* she remarks that it is "a man's tale, a tale of war," but that throughout the poem "women remind us . . . that human life goes beyond the wars of men, the founding of cities, and the building of empires" (p. 119). Saxonhouse, too, holds fast to the totality of human life, believing with the theorists she studies that the "regime" is more than simply formal political structures, and that even when women do not hold public office or take up arms, they are still an integral part of the social and economic relationships that constitute the larger community.

Showing that women do play a role in preliberal political theories does not, however, place these theories as far beyond feminist criticism as Saxonhouse sometimes appears to believe. For example, Saxonhouse qualifies Aristotle's pronouncement that women are "misbegotten males" with inferior deliberative faculties by noting that this natural hierarchy is often subverted, and that some women are more rational than, and hence should rule over, some men. Here Saxonhouse's concern to do justice to Aristotle and women alike excuses too much. And despite Saxonhouse's contention that preliberal theorists value women's activities and roles and often depict the family as resting on "a non-explosive, communal relationship between the male and female" (p. 80) one must ask whether nonexploitative relations are really possible within either the family or the polity, given women's exclusion from, and men's empowerment by, participation in the public realm.

Saxonhouse has shown that however much we, like Machiavelli, may wish to criticize the "masculine" in terms of the "feminine"—and the "feminine" in terms of the "masculine"—with an eye to transforming the very concepts themselves, we will err if we reject the values and activities traditionally associated with the family and women. But the preliberal "solution" of isolating women from the arena of public debate and political action cannot be our own. Saxonhouse's recovery of the classical thinkers' appreciation of the world beyond politics sharpens, rather than resolves, questions about the costs of a gendered division of labor and about how men and women should function as political creatures. For that very reason Saxonhouse's book makes a significant contribution to both the history of political thought and contemporary feminist theory.

MARY LYNDON SHANLEY
AND AUDREY MCKINNEY

Vassar College

Majority Rule. By Elaine Spitz. (Chatham, NJ: Chatham House Publishers, Inc., 1984. Pp. xi + 238. $12.95, paper.)

Elaine Spitz begins her study of the concept of majority rule by noting the surprising absence of literature directly concerned with that concept. Central to democratic theory and practice, majority rule is all too frequently assumed to be desirable per se, treated as mere procedure, or treated by its critics as a potentially tyrannical flaw in societies that deem themselves democratic. Spitz's intent is to provide a throughgoing analysis of the numerous constituent elements of the process of majority rule and to provide a justification for the centrality of that concept in any democratic society. She has succeeded admirably, producing a study that substantially fills the void she notes in the literature and that makes a genuine contribution to democratic theory.

One of Spitz's central themes is that standard discussions of majority rule tend to abstract the concept from the complex world of political decision making and to regard it as a device simply for recording and summarizing concrete preferences held by discrete individuals. In doing so, both critics and defenders of majority rule have been led to disregard the virtues of majority rule that arise from its characteristic practices of "inclusivity, discussion, periodic conflict resolution, and joint action" (p. 215). Hence, Spitz's study includes extensive and informed discussion of suffrage, citizenship, constituency, majority formation and duration, interest groups, and political parties as they combine to produce the milieu in which majority decision making operates. An entire section of the book is devoted to an analysis of the meaning of the word *rule*. Throughout, Spitz exhibits a wide-ranging knowledge of these often disparate subject matters and demonstrates ability to integrate them with her major concern.

Critics will disagree with some—perhaps many—points of her analysis in the first two sections of the work; for example, her disagreement with the Warren Court's apportionment decisions, or her belief that the problem of access to power for disadvantaged groups in a heterogeneous society poses no theoretical barrier to equal effective representation. Such disagreements would not disturb Spitz's major argument that majority rule must be viewed as a multifaceted and ever-changing process that performs often unrecognized functions in democratic societies.

The final section of the book is devoted to theories, criticisms, and justifications of the desirability of majority rule. Spitz argues effectively that majorities in democratic societies must possess total sovereignty in principle, although in practice that power is subject to delegation or self-limitation (pp. 109-115). Hence, attempts by prior critics of majority rule (Madison, Dahl, Gans, etc.) to limit its power by institutions, logic, constitutions, or the like are misplaced. Similarly, prior efforts to justify majority rule (Locke, Jefferson, MacIver, Kendall, etc.) are flawed or incomplete, because they neglect the communitarian and integrating aspects of majoritarianism, viewing it largely as involving the numerical summation of individual preferences. Finally, Spitz simply dismisses, perhaps too casually, the more radical critics of majority rule, who would argue for a true democratic community based not upon a conflict model but upon shared values, claiming that their vision is based on a "leap of faith" (p. 193).

To persons committed to the principle of majority rule but concerned about its potential excesses, Spitz offers, I fear, a leap of faith similar to that of the radical critics: She recognizes the potential power of the majority to tyrannize, but believes the power will probably not be used. Her belief is grounded in the values that arise from the complex processes of majority rule itself.

Spitz ends the book with her justification for the desirability of majority rule. Properly understood, the concept presupposes, generates, and supports appropriate democratic values, and "the virtues embedded in the practice of majority rule foster both individuality (the creative potential of each) and community (the unified action of all) in order to build a social world in which transcendent universal values can operate" (p. 215).

Readers will without doubt disagree with points of Spitz's *Majority Rule*, but they ignore it at their peril, for it is a first rate contribution to a central concern of democratic theory and practice.

DAVID E. INGERSOLL

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From Colonies to Commonwealth: Familial Ideology and the Beginnings of American Republic. By Melvin Yazawa. (Baltimore: The Johns Hopkins University Press, 1985, Pp. 261. $28.50.)

Over the past two decades, a renaissance in the study of the American founding era by historians has produced a secondary literature of high quality that must be read by anyone in political science serious about understanding American political thought. Historians like Bernard Bailyn, Gordon Wood, Jack Greene, Trevor Colbourn, Michael Kammen, J. R. Pole, and Jackson Turner Main, to name a few, have returned us to a careful and comprehensive examination of the pamphlets, tracts, letters, documents, and other public records which together provide the only valid basis upon which to base inferences and conclusions about early American political thought.

No longer is it intellectually tenable to do a close textual analysis of a single piece of writing from the era, even the United States Constitution, as if that single text were by itself sufficient for explaining its meaning. Far more so than is the case when analyzing major texts by European authors, texts in American political thought before 1800 are essentially "assembled texts," of which *The Federalist Papers* is but one example. The painstaking rediscovery of the broader political writing of the founding era, both in making these writings readily available and in providing coherent interpretations of their collective contents, is a process in which a good number of political scientists as well as historians have been engaged—witness the work of Herbert Storing, Charles Hyneman, and Martin Diamond, for example.

The work to date suggest that there is more coherence than was once considered the case at the same time that there is more diversity than was credited by earlier scholars. The relatively simple syntheses by Charles Beard, Carl Becker, Vernon Parrington, and Clinton Rossiter must be tested anew in light of what we now know about political thought in early America. One of the litmus tests for the adequacy of any such synthesis must now include an adequate accounting for the role of the concept of virtue. Badly misunderstood by many today because of its debasement by simplistic religious appropriation in recent years, the concept of virtue, we now know, was a complex, virile, and central component of early American political thought.

Despite the absence of the word in Yazawa's title, his book is in fact of the eighteenth century in America. The first debt political scientists owe Yazawa is his concise, comprehensive summary of what historians have to teach us on the matter. Skillfully working through the pamphlet literature and other sources, Yazawa provides a careful, balanced, and extremely readable summary of what might otherwise require a tedious reading of a voluminous literature, both primary and secondary, to attain.

Sometimes using concepts or forms of analysis borrowed from sociology and anthropology to complement his precise application of the standard techniques of the history of ideas, Yazawa blends it all into a fast-reading, jargon-free essay. The social science content is so unobtrusive that it must be looked for or missed entirely; but the net effect is to help produce an analysis that feels more realistic than is often the case with a pure history-of-ideas approach.

A second debt political theorists owe Yazawa flows in part from this quiet, effective blend of techniques. He is able to bring an understandable and credible coherence to what has increasingly become a source of confusion. How much weight should we give to the influence of the European Englightenment, the Scottish Enlightenment, the English Whigs (Court or Country), classical sources, or Protestant theology when searching for the origins of American political thought? His answer, indirectly arrived at, is: it depends. It depends on the problem with which you are wrestling, the part of the founding era you are discussing, and what the thinkers of the day felt was relevant.

The crux of Yazawa's book lies in showing the evolution in American political thinking from its domination by the metaphor of the family to its domination by an image of individuals as "republican machines." Put in less abstract fashion, Yazawa demonstrates the continuing importance of virtue for American political thought; but also how the communitarian, hierarchical perspective in which mutual dependence and affection served to define political virtue during the colonial era was replaced by a more individualistic and egalitarian perspective in which citizens are consciously educated to republican virtue.

Throughout the eighteenth century, Yazawa argues, a virtuous population was the bedrock upon which the American commonwealth was seen to rest. The filial basis of virtue served to underwrite all levels of politics, whether local or imperial. Further, this benign, developmental view of virtue inclined the English colonists in North America toward what we now call republican government. Rooted in theology, common law, and a peculiarly English approach to life in general, the familial paradigm of politics—when possessed by people in colonies distant from the mother country—had a certain logic leading toward equal, paternal treatment from England. When this bond of "affection" was not honored by the king and parliament, the logic of the paradigm led toward separation.

Gordon Wood in his seminal *The Creation of the American Republic, 1776-1787* (Chapel Hill: University of North Carolina Press, 1969), argues that the revolution first took place in the minds of the colonists. Yazawa materially advances our understanding of the underlying logic to that mental revolution. Too often historians have used a legalistic reasoning found in some American pamphlets to explain the logic of the revolution. Legal arguments had their effect, but people do not by and large engage in momentous and arduous activity for legal reasons. Yazawa shows how seemingly simple acts or omissions by the crown and parliament set off deep-seated repercussions in the minds of large numbers of Americans habituated by culture and circumstance to view their relationship with England in a familial context. Once breached, however, the familial metaphor was weakened for use as the basis for political virtue at all levels of government.

Furthermore, the underlying commitment to republicanism toward which the familial basis of virtue inclined Americans tended to undermine the bonds of affection upon which the familial perspective rested. Rapid advances in the theory of republican government, accompanied by improvements in republican institutions in a newly-independent America, led to the appropriation of ideas from Europe that shifted the view of virtue. American understanding of virtue became less communal and more specifically political. While still viewing politics and morality as inseparable, virtue came to be defined in more individual terms—

although, despite what Yazawa seems to imply, the communitarian view of politics was far from dead at the beginning of the nineteenth century. Yazawa quotes Benjamin Rush to the point—"Republican citizens must be attached not to each other primarily, but to the republic itself."

The need to attach citizens to the republic itself requires an educational system with specific characteristics, and Yazawa has a brilliant chapter summarizing American literature from the era on the topic of education. Working from the premise that human nature is the same in all ages, and similar causes will produce similar effects (Hume), Americans set out to produce a body of citizens capable of and willing to support republican government, a body of "republican machines." This literature on education is indeed voluminous, and another litmus test of future syntheses must be the incorporation of this body of writing usually ignored by those writing in American political theory.

Yazawa traces the skeletal outlines of a process with enough fleshing out to be convincing, yet the study is not exhaustive. If he is correct, then a rethinking of the "politics of deference" that is often attributed to colonial politics is in order. So must the contents of the Declaration of Independence be reconsidered, without relying upon only European sources. With this new understanding it can now be argued that even in *The Federalist Papers* one can see the notion of a virtuous people lying at the base of Madison's extended republic. Yazawa's work stands as a potential fulcrum in the study of American political thought. To a certain extent it is a summary and synthesis of earlier work, its own contribution to our understanding leads to a rethinking of many of the texts political theorists in our discipline sometimes analyze in isolation. It will be a blessing if those building upon Yazawa's insights will have the same economy of style and superb gift for using the pamphlet literature. Although the work suffers a bit from the current tendency of historians to view all political ideas as simply ideology without concern for their truth value, this is still an important book, and should not be ignored by those outside the author's discipline.

DONALD S. LUTZ

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Locke: An Introduction. By John W. Yolton. (Oxford & New York: Basil Blackwell, 1985. Pp. xii + 162. $29.95, cloth; $9.95, paper.)

It is not an easy task to write a brief introduction to the thinking of a great philosopher, let alone to the thinking of one whose interests were as diverse and whose historical impact was as complex as those of John Locke. John Yolton's credentials for undertaking the task are as evident as they are solid. He has lived in some intimacy with Locke's *oeuvre* for well over 30 years. His writings on this *oeuvre* stretch from the admirable *John Locke and the Way of Ideas* (Oxford, Clarendon Press, 1956) to the two stimulating discussions of the subsequent fortunes of key portions of the *Essay* (*Perceptual Acquaintance from Descartes to Reid* and *Thinking Matter: Materialism in Eighteenth Century England*) that he published in 1984. Recently, too, he has taken over the mantle of Peter Nidditch as general editor of the *Clarendon Edition of the Works of John Locke.*

Any attempt to interpret thinking of real intellectual importance in the past is subject to two very different hazards: the risk of failing to capture the sense of its subject's understanding from his or her own point of view, and the risk of failing to convey just what that understanding, seen from the very different perspective of today, really signifies. No account of past thinking of any real power can reasonably hope to succeed perfectly in avoiding both hazards. Few, in fact, are conspicuously successful in avoiding either. The safest introduction to any thinker, as Yolton cannily notes and as Locke himself took the precaution of pointing out in the epitaph he composed for his own tombstone, is that thinker's own works. In the case of Locke these are voluminous in scale, and their purpose is not always very transparent to the modern eye. What Yolton sets himself to do—and on the whole does very well—is constrict the scale sharply and somewhat increase the transparency. His is a gracious and serviceable introduction but not, perhaps, a tremendously commanding one. He is better at performing the introduction itself than at conveying firmly to his readers just why they should require or welcome being introduced to Locke at all.

In practice today many different types of audiences are apt to have occasion to encounter one aspect or another of Locke's thinking. These different audiences (students of several branches of history, of philosophy, political theory, education, divinity, psychology, linguistics) naturally require rather different sorts of introduction. An ideal introduction to Locke for a modern philosopher, for example, is unlikely to be perfectly suited to the needs of a student of the English ecclesiastical establishment and its seventeenth- and eighteenth-century vicissitudes. Yolton confronts the problem of intellectual design posed by this miscellany of audiences by resolutely ignoring it. He presents Locke to his readers not as a figure in one or another course of contemporary university instruction, still less as an insistent imaginative presence in modern thinking about any particular issue, but rather as an old and valued friend, seen sternly but not ungenerously with all his foibles and infirmities of character.

It is not that Locke's doctrines themselves are presented in a wholly uncritical manner. A few discreet doubts are raised about the adequacy of some aspects of his epistemology and cognitive psychology, and one rather severe judgment is offered in the book's concluding sentence about his limited capacity for theoretical or practical empathy. On the whole however, Locke's doctrines are left to speak quietly and plainly for themselves. The most interesting and effective element in their presentation is the stress laid upon the centrality of the concept of person in both his ethics and his metaphysics, and upon the role of mental activity in determining the content of human belief.

The absence of any very strenuous consideration of the validity of most of Locke's philosophical doctrines is unlikely to commend itself to most contemporary philosophers, for whom the perfect introduction to Locke is apt to be a presentation of his doctrines designed to bring out their marked shortcomings when measured against the deliverances of a good modern philosophical education. At a more relaxed level, much the same is likely to be true for students of political theory, for whom the question of which, if any, elements of Locke's understanding of the nature of politics are still in fact esentially sound is also naturally of some interest. There are many sorts of introductory needs for which Yolton's book will be of little, if any, use. Especially, and gratuitously, unsatifactory is the very perfunctory coverage of the range and quality of the modern secondary literature. Yet for the rather more advanced student (particularly the one caught up in the thick of modern disputes about intellectual issues on which Locke's thinking has left a lasting impress) this modest, direct, and scholarly account will be an admirable corrective to anachronism and a steadying influence upon the judgment. Locke himself would have had good reason to be grateful.

JOHN DUNN

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AMERICAN POLITICS

The Cross, the Flag, and the Bomb: American Catholics Debate War and Peace, 1960–1983. By William A. Au. (Westport, CT: Greenwood Press, 1985. Pp. xvii + $29.95.)

William A. Au shows clearly that American Catholics are newcomers to the debate of war and peace issues in his excellent book, *The Cross, The Flag, and The Bomb.* Prior to the Second Vatican Council's plea for a "new vision of peace," only two groups within American Catholicism were concerned with the peace issue: The Catholic Association for International Peace (CAIP), founded in 1927, working primarily from the just war tradition, and the Catholic Worker movement, which first proclaimed its pacifism during the Spanish

Civil War. Both groups, like the papacy, were more concerned about international issues rather than national ones.

The willingness of American Catholics to criticize their nation in the 1960s and 1970s was a significant departure from the American Catholic tradition of trying to be superpatriots. Dorothy Dohen has described this strain of Catholic patriotism in her book, *American Catholics and Nationalism*. The new attitude of Catholics resulted in a new ethical pluralism in the debate on war and peace issues. Au devises a conceptual framework in which he classifies this ethical pluralism into four conflicting schools of thought: Catholic realism, nuclear pacifism, pacifism, and resistance.

After reading the positions, the reader is amazed by the absence of theologians from the debate of war and peace issues. After the death of John Courtney Murray, S.J., individuals such as William V. O'Brien, Justice George Lawler, Gordon Zahn, and Philip and Daniel Berrigan were the leading spokesmen of the conflicting positions. None were theologians. Also noteworthy is the increasingly critical attitude of the American Catholic hierarchy toward the United States government's defense policies and the draft, the culmination of which was the 1983 pastoral letter, *The Challenge of Peace*.

Au explains how *The Challenge of Peace* avoided a direct confrontation between church and state on the nuclear defense issue. He also shows how the bishops, in striving to attain a moral consensus, ultimately avoided endorsing any one of the aforementioned four opposing schools of thought. Au attributes this accomplishment to the efforts of Reverend J. Bryan Hehir, principal architect of the pastoral letter, who, though himself a nuclear pacifist, endorsed the ethical pluralism of American Catholics.

Au, however, sees nothing to celebrate in the new ethical pluralism. He prefers to be the prophet of doom for whom the conflicting schools of thought are on a collision course that will alienate American Catholics from either their church or their nation. Au fails to stress the achievements of consensus reached by Catholics on issue of war and peace. One of the most important of these achievements was the right of individual conscience as it relates to conscientious objection and selective conscientious objection. This was an historic development that was achieved in the 1960s for the first time in the history of the Catholic church.

This is a fine analysis of the ethical issues of war and peace. My only quarrel with the author is his desire for an intellectual consensus on such a complex issue. In a world of theological and cultural pluralism, such unanimity of thought on ethical issues is highly unlikely.

PATRICIA MCNEAL

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Discrimination, Jobs, and Politics: The Struggle for Equal Opportunity in the United States since the New Deal. By Paul Burstein. (Chicago: The University of Chicago Press, 1985. Pp. x + 247. $30.00, cloth; $12.95, paper.)

The legacy of social policies of the 1960s and 1970s concerned with discrimination and economic opportunity for those on the margin of social, political, and economic life is much discussed, but little understood. Opinions abound. Case studies proliferate. Techniques of data collection and manipulation seem barely altered over time. Comprehensive analysis is confounded by disciplinary barriers—yardsticks, frameworks, theories, and conventions—the language, logic, and liturgies of the not quite social sciences that tempt few to step beyond their specialized niches. Burstein is an exception. A sociologist by training with a track record in the sociological study of politics that is truly remarkable, he has brought together in one volume three fine elaborations of previously published studies concerning employment-discrimination legislation. The book enriches our understanding of the interrelationship of complex factors, and will enliven future debate. Where he is informed by sound theory and exercises his high standards for exhaustive data manipulation, he is first rate. Where he flounders are in those areas where theory is highly contested, particularly economic models concerning women. There his scholarship becomes shaky, his conclusions forced.

Burstein's landscape is broad, and his approach to political sociology meticulous. He begins with the tension between the promise of fairness and the experience of discrimination that led to the organized struggle for equal employment opportunity legislation at the federal level. He self-consciously refuses to consider only periods of crisis surrounding the time that Title VII of the Civil Rights Act of 1964 and the Equal Employment Opportunity (EEO) Act of 1972 passed, and laboriously sets out to catalog another agenda: that this legislation is a direct legatee of the New Deal. He develops a coding system to analyze the hundreds of EEO bills introduced from 1942 on. His aim is to produce the "first attempt to use systematically gathered time-trend data to show how public opinion and the civil rights movement affected congressional action on EEO" (p. 72). He presents twelve or so arrays of data beyond this codification of EEO bills, including congressional sponsorship, attitude surveys regarding minorities and women, analysis of incidence of State EEO and Equal Pay laws, trends in judicial rulings of the Supreme Court, comparison of support for civil rights in principle and practice, incidence of riots, demonstrations, media coverage, and lobbying that may have affected the timing and passage of Title VII and the EEO Act of 1972.

He finds the EEO law essentially conservative. It was "respectful of property and of business, oriented toward conciliation, employing elaborate procedures, focusing on individual cases" (p. 186), hardly a radical trade-off for the cessation of social protest. Why had it passed? What effect had the civil rights movement and these other factors on congressional action? Burstein concludes that public opinion was the fundamental determinant of congressional action. The civil rights movement affected Congress in two ways: drawing attention to the issue of civil rights, and provoking a violent responce that increased the salience of the issue for the public. Anti-rights activities had reduced congressional support, but they also increased the salience of civil rights for the public. The effect of these two was not large in Burstein's view, and he considers it impossible to say who gained the most. Essentially it was the power of the first draft that effectively set the terms of future congressional action. It was essentially an idea whose time had come, that took the form of proposals that had been in existence for 30 years.

The second half of the book dealing with attempts to assess the impact of EEO legislation on the economic status of nonwhite males and females and of white women is limited in two respects. There is no attention given to the extraordinary reorganization of the U.S. Equal Employment Opportunity Commission (EEOC) structure and functions that occurred during the Carter and Reagan administrations, nor to subsequent Executive Orders, or EEOC directives concerning implementation. Burstein states that "even the most radical members of Congress show no sign of developing new legislative ideas to hasten the end of employment discrimination. And there is little pressure on them to do so" (p. 36)—a remarkable observation in the light of a decade of litigation and legislative activity on comparable worth. His construction of a measure of proportionate wealth called *group share* seems similarly misguided in overlooking current research dealing with the economic status of women. It remains an interesting and useful book, despite these reservations; ambitious and certain to stir further research.

ELAINE JOHANSEN

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Socialism and America. By Irving Howe. (New York: Harcourt Brace Jovanovich, 1985. Pp. 24. $17.95.)

Socialists and the Ballot Box: A Historical Analysis. By Eric Thomas Chester. (New York: Praeger Publishers, 1985. Pp. 192. $28.95.)

These are very different books. Howe's book tells us a great deal about socialism, liberalism, and America, while Chester offers a narrowly focused historical critique of the American Left's tactics and intends to speak only to a rather small audience. But although quite different, these books actually complement each other. This is because Chester seeks to explain, and to assert the limits of, Howe's politics. Chester assesses the tactics of the Democratic Socialists of America (DSA), a somewhat influential outgrowth of what was left of the organized Left in the 1960s. Howe and his colleague Michael Harrington (one of the two men to whom Howe dedicates his book) are named on p. 10 of Chester's book as important targets of his critique, for both play important roles in the DSA. And in order to mount his critique Chester treats obscure but still quite interesting corners of the American Left's history. Chester thus sketches in the specific intellectual historical and organizational context of some of Howe's politics.

Still, Howe's analysis has an integrity all its own. The book is not a political tract at all—it is a cultural and social criticism. Howe breathes significant and vivid life into questions which can be and often are dismissed as settled, trivial, or wrongheaded: What explains why the United States does not how have a mass socialist party? What are the relationships between socialism as a theory and liberalism? What would liberal socialism or socialist liberalism be like in institutional practice?

Howe's discussion of the first question is a valuable cross between "national character" and historical organizational analysis. Lucidly written and psychologically perceptive, it is perhaps the best short introduction to the history of American socialism between 1900 and 1940, and to the existing scholarly perspectives on it.

Without ever calling it this, Howe offers, I think, a "track" view of American exceptionalism. Between 1912 and 1919, just as it became moderately successful, the Socialist Party faced a series of organizational challenges that it did not adequately meet. The responses to these challenges locked the party into an organizational limbo. Over time it became less movement-like and more sect-like. Even the Depression failed to revive it organizationally. It became only one among several left movements that were themselves small and organizationally fluid. In short, because the Socialist Party did not take hold during industrialization, the dice were loaded against it during the crisis of industrial capitalism. Ironically, the Communist Party made more of this crisis, but its subordination to the Comintern's policy twists made its success little more than a "brilliant masquerade," to use Howe's term for the Popular Front. The Popular Front was an ersatz version of mass influence that underscored the fundamental reality of the left's impotence; that is, its lack of an organic, historically created relationship to American politics. In all, Howe suggests, this is a story of non-development, of socialist politics being "tracked" away from the possibilities for political influence embedded in industrial society.

What, however, of the present? Even though Debsian Socialism never became a mass movement can the politics of democratic socialism say anything to contemporary liberal America? Howe spends the rest of his book on this issue, offering a subtly constructed argument that democratic socialism and liberalism actually sustain each other, in significant part due to the major innovation of the twentieth-century American political economy—the emergence of the welfare state.

The title of Howe's book thus has a different meaning in the second half. Howe daringly suggests that socialism and America have an as yet unknown and linked history. Starting with an analysis of a failed past, he ends with a plausible sketch of a possible future. Socialism may yet be in America's future. Democratic socialists may yet nudge liberal social democracy along, using argument and public debate, toward a polity in which hierarchies rooted in the market become less important than they are now.

This nudging type of politics, however, is precisely what Chester objects to. He subjects Debsian Socialism, the Popular Front, American Trotskyism, and the politics of the DSA to an historical and tactical analysis designed to show that militance rather than nudging has been consistently underestimated on the American left.

Chester's advocacy of militance is axiomatic. He assumes that social democracy and unbridled capitalism are equally repressive without ever quite saying why. Chester thus tends to picture nudging simply (and implausibly) as a failure of imagination and will.

Nevertheless, Chester has rather good historian's instincts. He picks up the story where Howe leaves off and in fact offers the first serious account we have of the origins of the contemporary American Left. These go back to the preparations that independent Trotskyists made during World War II for shaping postwar American social democracy. A group of highly anti-Communist American Trotskyists led by Max Schachtman gained practical experience in union organizing and in the institutional foundations of American social democracy. They did this by establishing a foothold in the United Automobile Workers during World War II and staying there, a feat made possible by long familiarity with unions and how they work. This experience set in motion events that culminated during the 1970s in the establishment of both the Social Democrats, USA (a group that has exercised considerable influence within the Reagan Administration) and of the Democratic Socialists of America. Chester thus offers very suggestive, useful glimpses into the institutional origins of some of the ideological impulses in our decade.

One book, then, should be in anyone's library, despite the disagreements one might have with it. Howe is a very stimulating writer. As for the other, probably only an institutional library should acquire it. There, it will be of help to certain scholars, some of whom, one hopes, will have just come from reading Howe.

RICHARD VALELLY

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Governing the Ungovernable City: Political Skill, Leadership, and the Modern Mayor. By Barbara Ferman. (Philadelphia: Temple University Press, 1985. Pp. xii + 281. $34.50.)

This book is, as the title of the second chapter would have it, a tale of two cities. It offers lengthy case studies of politics in Boston and San Francisco during the 1970's. Since these are—for good reasons—favorite cities among political scientists, and since the case studies are detailed, comprehensive, and very interesting, virtually all urbanists will enjoy reading this work.

The substantive focus is on mayoral leadership. As a result, what we really have are histories of, in Boston, the White administration and, in San Francisco, the Alioto, Moscone, and (more briefly) Feinstein administrations. The author seeks to explore the various ways in which these individuals sought to acquire, maintain, and enhance their own political power. She finds that all of them relied, to a greater or lesser degree, on strategies that fall into five basic areas: the use of conflict, personnel policy, government reorganization, opportunities presented by federal grant programs, and the development of large-scale public building projects. The case studies are organized, and intertwined, in terms of these five categories. Thus, for example, chapter 4 on personnel strategies, looks at Kevin White's efforts to gain power first by recruiting progressive, good-government types (such as Barney Frank) and later by turning to the tactics of machine politics. The chapter then compares these with the appointment strategies of Alioto and Moscone, which were oriented more toward organized pressure groups. This mode of presentation is effective. It allows the author to contrast the two cases at every relevant point, and also to develop something of a theory of urban leadership. The theory itself strikes me as quite unremarkable. The various strategies, she says, are more or less effective, depending on the mayor's political skills, the city's political culture (public vs. private-regarding), and the political system (reformed vs. nonreformed). While not especially novel, such a view is however, particularly plausible when presented in the context of a typology of strategies and a wealth of case study material. In this sense, then, the book certainly is a significant addition to the analysis of leadership in urban politics.

It is with the case studies themselves that very serious questions arise. The literature of case studies in urban politics has, of course, been deeply concerned with issues of method and evidence. Such a concern is hardly in evidence here. The author explains that the study is based primarily on 123 interviews, roughly split between Boston and San Francisco. She tells us virtually nothing about who was interviewed, how they were selected, what kinds of questions were asked, or why the interviews would be (equally?) believable as evidence. This problem, troubling in itself, is particularly so in the light of the author's fundamental, though generally tacit premise; namely, that most of the things each of the mayors did can be fully explained as an effort to maximize personal power. Consider the following passage on Kevin White:

> White's selection of the race issue as a major electoral appeal was strongly influenced by his per-

> sonal ambitions for higher office. . . .When White ran for mayor in 1967, he hoped that the office would serve as a stepping stone to the gubernatorial race in 1970. . . . By adopting the race issue, White simultaneously appealed to the black community, which was not represented by any of the other candidates, and to liberal suburban voters whose influence in state politics increased as party lines became more tenuous. The pro-minority stance represented a sharp departure for a Boston city politician. This bold move enhanced White's appeal among his two target groups and helped him to sew up their support early on. (p. 55)

We have here a quite cynical picture of a man and his deep motivations, presented without a trace of evidence. I have my own views of Kevin White and, in fact, they do indeed comport with passages such as this. However, these are merely views, not empirical evidence, and I would be most hesitant to use them as a base for a social scientific theory of leadership in urban politics.

PETER J. STEINBERGER

Reed College

The Federal Government and Urban Housing. By R. Allen Hays, (Albany, NY: State University of New York Press, 1985. Pp. xvi + 297. $39.50, cloth; $14.95, paper.)

This work provides a thorough analysis of federal intervention in the U.S. housing market from the 1930s to present. In doing so, Allen Hays attempts to demonstrate how the ideological orientation of each presidential administration influences both the design of federal housing policy as well as the level of federal commitment. In chapters 1 and 2, Hays develops a theoretical framework for analyzing the general role that political ideology plays in shaping public policy and how ideology specifically influences housing policy. Chapter 3 provides a comprehensive and well-balanced analysis of changes in the housing needs of U.S. families. The issues of affordability (measured as a percentage of family income paid for shelter), changes in housing quality, and the causes of housing inflation of the 1970s are carefully analyzed in this section.

Chapters 4-6 examine each major low-income housing subsidy program beginning with public housing through the current section 8 housing assistance program. The author takes issue with those who argue that the homeowner and rental assistance programs established under the Johnson administration (sections 235 and 236) were failures. He provides evidence showing that even though the financial risk involved in these programs was greater than the risk found in more conventional housing assistance program (e.g. FHA mortgage insurance), the failures were blown out of proportion by critics of the programs.

Chapters 7 and 8 focus on federal involvement in urban redevelopment. Again using political ideology as the major determinant of policy formulation and change in this area, Hays begins with the Urban Renewal Program adopted in the Housing Act of 1949, and traces urban revitalization programs initiated by the federal government up to the 1974 Community Development Block Grant Program. Here Hays shows how urban renewal moved from an emphasis on neighborhood destruction in the 1950s and 1960s to a greater emphasis on neighborhood preservation in the 1974 Community Development Block Grant Program.

While Hays is likely to find general support for the argument that liberals and conservatives generally differ in their views about the role of government in the private market as well as about the role that the federal government should play in providing housing assistance for the poor, this glosses over "the coalition building process" that more accurately describes the "winners" and "losers" in the housing policy arena. For example, if U.S. housing policy were truly determined by the ideological orientation of each presidential administration, then one would expect that Mr. Reagan would have found more support in his own administration for his effort to do away with the mortgage interest tax deduction as well as with FHA. Despite the fact that these programs are ideologically inconsistent with the Reagan administration, they are favored by interest groups that are integral parts of the Reagan coalition.

As described in this book, "changes in the ideological orientation of policy-making elites brought on by changes in presidential administrations" serve as the major explanation of changes in U.S. housing policy. Even so, the author is compelled to acknowledge the influences of rising construction costs, changing housing needs, and program inequities that led many policy analysts to lose confidence in the low-income housing subsidy programs, developed during the Johnson administration, in favor of programs requiring less direct government intervention (e.g., housing vouchers).

Moreover, in his discussion of urban renewal, Hays shows that even ideologically conservative inner-city businessmen supported government intervention in urban renewal programs, in order to save their investments in the inner city. This discussion once again demonstrates the limitations in the use of the concept *Political ideology* to explain support for federal intervention in the private market.

Overall, this book succinctly and understandably summarizes U.S. housing and community development policy, and provides insight into the complexity involved in policy making in this domestic policy area. Even though the work relies heavily upon secondary sources and does not break much new ground in the housing and community development field, it does provide a timely analysis and critique of policy developments in this rapidly changing area.

BYRAN JACKSON

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The Election of 1984: Reports and Interpretations. Edited by Gerald M. Pomper and Marlene Michels Pomper. (Chatham, NJ: Chatham House Publishers, Inc., 1985. Pp. x + 197. $20.00, cloth; $8.95, paper.)

Visions of America: How We Saw the 1984 Election. By William A. Henry III. (Boston: The Atlantic Monthly Press, 1985. Pp. ix + 275. $17.95.)

Presidential elections are the most thoroughly studied political events in modern times, and these books exemplify the "instant analysis" we have come to expect in the last two decades. The first represents analysis by political scientists and the second by journalists.

Gerald Pomper and associates at Rutgers University have previously reported the 1976 and 1980 elections; this book is the third in a series that is likely to continue. The election story is told, but is clearly subordinated to analysis. The data on which the analysis rests are polls and election returns.

Of the nominating process in 1984, Pomper says that "President Reagan's renomination marked the transformation of the Republican party into a cohesive organization with a coherent ideology" (p. 1), and that in their Dallas convention the "Republicans were mired in agreement. . . . [They] stretched to fill the free media time" (p. 28). The Democrats once again had changed their rules (the Hunt Commission), this time in favor of an establishment candidate. They had their usual "carnival" nomination struggle, and Mondale "won the nomination in the end because he was the most consensual candidate available" (p. 23). The Democrats had what was for them a remarkably unified and successful convention. Jesse Jackson's candidacy for the nomination and Mondale's selection of Ferraro as his vice-presidential running mate were "firsts" in 1984, with enormous long-run potential importance.

Henry A. Plotkin discusses 1984 campaign issues as "the war of ideas" between the candidates, which rested most notably on their contrasting visions of America. Public confidence in economic recovery overshadowed Mondale's issues of "fairness" and "bad news" warnings about deficits. Considering a wide range of other issues, Plotkin concludes that though many of the voters disagreed with Reagan on particular issues, he "has succeeded in shifting the political agenda of the nation significantly to the right" (p. 56).

Pomper assesses several possible interpretations of the election, including that 1984 "may have moved the nation toward a new conservative and Republican alignment" (p. 60). Mondale, he points out, "won only among blacks, Hispanics, Jews, members of union households, residents of large cities, and persons either unemployed or with the lowest family incomes . . . added together, they constituted only a minority of the total population" (p. 66). The Democrats have good reason to worry about future prospects. First, the South, once solidly theirs, voted more heavily for Reagan than any other section. Second, the children of the "baby boom" did not take on their parents' party identification, and youth turned toward the Republicans in 1984. Finally, the Democrats seem not to have mastered the campaign technology to reverse the secular decline of party loyalty nearly so well as the Republicans.

Scott Keeter presents public opinion data showing an "upbeat mood" among the people that would not be offset by any forecast of economic difficulties to come: (1) the people liked Reagan personally and perceived him to be a strong leader. (2) the economic indices from 1981 to 1984 almost perfectly fit Reagan's approval ratings, (3) Mondale's issues of "fairness" and "peace" simply could not make a dent in the "good news" campaign of the Republicans, and (4) the "gender gap," from which the Democrats expected a bonus in votes from women, especially considering the Ferraro candidacy, only slightly reduced the margin by which women voted for Reagan, although it may have been of sufficient help to decide some state and congressional contests for the Democrats. Ideologically, "one finds only a glacial shift to the right" (p. 108).

The incumbent advantage, and the fact that very few members of either the House or the Senate retired in 1984, explain, according to Charles E. Jacob, the near-status quo outcome of the congressional races. The very small freshman class in the Senate is more moderate or liberal than the group it replaces. The Republican portion of the equally small freshman class in the House is quite conservative, but the House is still in control of the Democrats.

The conventional wisdom, according to Ross K. Baker, is that "prospects for second-term Presidents are not very bright" (p. 133), and that Reagan seems bedevilled by a want of a clear mandate (because nothing much was promised), a host of economic problems, foreign policy dilemmas, the Democrats, and the potential for a split between the pragmatists and ideologues in his coalition. However, Reagan has the possibility of naming several members of the Supreme Court, which could satisfy the ideologues. He has the ability to focus on the part of government that fits his personal mission ("3-D government of defense, diplomacy, and deficit) and ignore the rest, and has begun a concerted effort to infiltrate the bureaucracy that has been so long dominated by liberals.

Writing on the meaning of the election, Wilson Carey McWilliams says of 1984 that "Americans wanted a moment of relaxation, and Ronald Reagan, campaigning as 'Dr. Feelgood,' knew better than to prescribe any bitter medicine" (p. 158). Reagan, McWilliams says, ran as a moderate, ditched his party, and was reelected along with many Democrats in Congress because the people desired no great change. He argues that "Mondale is visibly a man of the old order" (p. 164), and that the old pluralistic politics of coalition building are played out, along with the New Deal strategy. McWilliams proposes a scheme for restoring key elements of the New Deal coalition to sufficient allegiance to win again nationally.

The weaknesses in this book, which are few, arise from it being a symposium and from the speed with which it was written and published. The chapters are a bit uneven, and there are occasional disagreements between authors. Despite these shortcomings, this is an excellent continuation of the series. It places the election in a theoretical perspective and it reached professionals and their students without the inordinate delays associated with most publishing schedules.

William A. Henry III is one of a current generation of journalists who have decided to follow in the footsteps of Theodore White and H. L. Mencken in reporting American presidential contests. *Visions of America* is his first effort, and a worthy one. In the preface, Henry says "the story ought to be a narrative" and "that the real campaign is the string of public events that voters observe, not the hidden web" (p. vii). Other, recent journalistic accounts have gotten inside the campaign, relying on both interviews with the strategists and their memoranda to such a degree that they have become quasi-participant-observer accounts. Such is not the case with this book. If you want to know in some detail how Reagan and Mondale prepared for their debates, you will not find it here. Nor will you find out how their staffs reacted backstage to the debaters' performances. There is little "name dropping" of advisers in the book, and almost no appreciation of behind-the-scenes theme-selection or strategy-building. In fact, on only a few occasions does the reader perceive that the author was present at important campaign events. Most of the information conveyed in the book could have been collected by watching television, reading the press, and checking a variety of secondary sources (including *Time* magazine, for which Henry is an Associate Editor). Like a good story, the book is well written. It has no footnotes, no bibliography, and no appendices.

The bottom line, as Henry reports it, was that all of the Reagan operatives believed the president was unbeatable, and although the Democrats thought there were five ways they might be victorious, none of their options was probable. Almost everything went right for the President. Henry excellently sums up Reagan and his effect on America:

> Reagan apparently had the gift of making people feel good about the country—and the even greater gift of making people feel good about the rich. He dispelled class resentments; he eased the pangs of conscience among those who have toward those who have not; he reconciled his countrymen to inequality, and absolved the prosperous of any hint of shame at making gaudy display of their riches. (p. 209)

As for the Democratic team, once it had silenced most of the Ferraro tax return business, it was beset by the Catholic bishops on the issue of abortion and doomed to a never-ending search for an issue or a theme that could make the Democratic challenge competitive. The first debate (presidential) became the "most influential moment of the campaign" (p. 244), because Reagan did not live up to media expectations, and his bumbling performance permitted journalists to discuss openly the issue of age and competence. The second (vice-presidential) and third (presidential) debates were judged on the basis of different media expectations. In the rematch with Mondale, Reagan won the "sound bite," the best one-liner of the evening, and with it, reelection by a landslide. The people preferred Reagan's "vision of America."

Although *Visions of America* is atheoretical, it goes well beyond the standard "who, what, when, and how" of traditional journalism to treat "why" the 1984 election went the way it did. Accordingly, it is a welcome addition to the literature on one of the most complex political events we political scientists have the responsibility to explain.

DONALD M. FREEMAN

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Without Justice for All: The Constitutional Rights of Aliens. By Elizabeth Hull. (Westport, CT: Greenwood Press, 1985. Pp. xi + 244. $29.95, cloth.)

Citizenship Without Consent: Illegal Aliens in the American Polity. By Peter H. Schuck and Rogers M. Smith. (New Haven: Yale University Press, 1985. Pp. viii + 173. $22.50, cloth; $6.95, paper.)

Moments of political crisis tend to provoke statesmen and theorists to reexamine the fundamental principles of a political community. So it appears that the recent "crisis" surrounding the influx of illegal aliens and refugees into this country, and the apparent loss of control over our borders, has stimulated a political resonse in the introduction of the Simpson-Rodino bill in Congress and an academic response as evidenced by these two brief books. These studies of the rights of aliens and the underlying premises of our immigration policy raise the broader question regarding the nature of citizenship itself. Indeed, as a nation with a long tradition of unrestricted immigration, America has seldom been forced to formulate any specific notion of citizenship other than at the time of its founding and during the recurrent strugle over the citizenship of its black population. With the imposition of the first immigration laws, the polity was forced to articulate a basis for inclusion into the political community for outsiders. While our native literature has told us what it means to be an "American," our political theorists including the Founders have left us uncertain about what it means to be a citizen. Recent confrontations with the problem of illegal aliens and refugee groups suggest that we have yet to articulate a coherent theory of citizenship.

In *Without Justice for All,* Elizabeth Hull focuses her attention on the constitutional rights of aliens. This is a highly readable and informative account of U.S. immigration law and policy, with the central concern the political status afforded aliens—both resident aliens and those who have entered the country illegally. That political status and the protection of constitutional rights is considerably less for illegal aliens. Also legal resident aliens who have entered the country under the terms of the Immigration and Naturalization Act do not enjoy the same constitutional rights as citizens.

Even while the Constitution itself makes certain guarantees to all "persons." the question is whether Fourth and Fifth Amendment rights, due process, and equal protection attach to citizenship only, or are granted to any person legally under the jurisdiction of the U.S. government. The Supreme Court has consistently recognized some restrictions upon the rights of aliens (beyond the requirement of citizenship for holding high office specifically mentioned in the constitutional text). However, in *Graham v. Richardson* (1971) the Court recognized alienage as a "suspect" category, at least in terms of receiving state and social welfare benefits. The tone of Professor Hull's book suggests that she favors a broad movement in this policy direction. Indeed, one gets the impression that she opposes any attempt to distinguish between aliens—whether legal or illegal—and citizens, or to make any discriminations regarding who may enter and join the political community. The implication of such a view is much the same in the Court's holding in *Graham*—it "marks an important milestone in the devaluation of citizenship," as Schuck and Smith suggest (p. 107). Hull's book is a useful guide to understanding U.S. policy toward aliens; however, one may reject the author's attempt to sever the notion of constitutional rights from citizenship. In looking to evaluate the rights of aliens to those of citizens, it is to the Court that Hull inevitably turns: "This country's alien population would benefit significantly if the Supreme Court intervened more actively in its behalf" (p. 151). There is no doubt of such a benefit; the question is whether it is appropriate.

Schuck and Smith recognize the legitimacy and right of a polity to determine the terms of inclusion into its membership. The authors fruitfully spend much time explicating the notion of citizenship in liberal political thought and American legal theory. This effort generates a formulation of two distinct models of citizenship—one ascriptive and the other consensual. While the latter is viewed as more appropriate to a liberal polity, the authors suggest the historical prevalence of the ascriptive model in which the circumstances of one's birth determine citizenship. They outline an alternative approach incorporating a Lockean notion of "tacit consent" thereby emphasizing consensual elements even while recognizing birth as initially determinative.

This theoretical study of the historical origins of these different notions of citizenship and the extrapolation of a concept of citizenship from the U.S. Constitution, the 14th Amendment, and Court decisions is excellent. While the book as a whole is presented as a timely discussion of recent debate over immigration law and policy, its greater value lies in its contribution to understanding the theoretical underpinnings of citizenship and the basis for inclusion into the polity. The publisher offers this work as part of a paperback series designed to "bring subjects of keen current interest to readers as quickly as possible." This is appreciated; however, a topic as important as this deserves an even fuller treatment. One hopes the authors will provide it in the future.

SHELDON D. POLLACK

University of Pennsylvania

To Serve the People: Congress and Constituency Service. John R. Johannes. (Lincoln, NE: University of Nebraska Press, 1984. Pp. xv + 294. $21.95, cloth; $10.95, paper.)

Appropriately enough, the very appearance of this review of *To Serve the People* results from "casework," the subject Johannes examines in this book. The publisher discovered that an original review copy had not reached the *Review*. Another was sent, and the regular process went forward—one small instance of rectifying an error or solving a problem, which is what most casework is all about.

Congressional casework is inescapably mundane, and perhaps the greatest virtue of *To Serve the People* is that Johannes never forgets this central truth. At the same time, casework is neither simple nor unimportant. Rather, it meshes, often seamlessly, with the remainder of congressional life—legislation, reelection, oversight, relations with the executive, and so on. In addition, casework bridges the district-Capitol Hill gulf. In that context, this book provides a useful supplement to Fenno's *Home Style* (Boston: Little, Brown, 1978) by demonstrating how members and staff divide their responsibilities and deal with their Hill and home constituencies.

The book's straightforward organization moves from a relatively limiting definition of casework through discussions of the extent of caseloads and how cases are typically resolved. Subsequently, with a heavy (and appropriate) emphasis on both congressional and executive actors, Johannes examines the structure and process of casework. Many of the book's most valuable insights come in his reporting of how agency and administration personnel react to various forms of congressional entreaties.

The volume continues with three chapters that analyze the effects of casework upon (1) the equitable resolution of constituents' issues, (2) policymaking and oversight, and (3) incumbents' reelection success. Johannes concludes his study with a rather choppy chapter that attempts to assess casework within a broader, ombudsman framework. This approach does not so much fail as appear a bit out of place; by the end of the book the reader is scarcely looking for universal conclusions.

Although Johannes has been involved in some heated exchanges over the impact of casework on election outcomes, this subject receives only modest attention in the book. Of greater importance is the way in which the author combines statistical analyses with a rich collection of interview and questionnaire data. *To Serve the People* stands as an excellent example of how to weave different kinds of data into a coherent whole. It appears that Johannes made a virtue of necessity here. The statistical findings, mostly from probit analyses, are far from overwhelming. Rather than overemphasize some modest results, Johannes chooses to underscore the complexities inherent in the casework process. For example, both congressional staff and executive-branch actors observe that obtaining favorable results for one individual may mean that others in the constituency will come out losers. Casework may not quite represent the simple "pure profit" that Fiorina has claimed.

Upon completing this book, I felt I knew a good deal more about casework than I had beforehand. More importantly, I can think more coherently about casework than I could previously. From this perspective, *To Serve the People* will serve its constituents—students of the Congress—well for a long time. Johannes had the good fortune to begin his research in the wake of the major changes in staffing levels. Although automation will change the nature of casework, the basic outlines of the process were firmly in place by the late 1970s and early 1980s. For example, most casework had been moved to the district, at least by younger members, and Johannes is able to take this into account.

Congressional scholars could do a lot worse than to emulate this book's approach to "the New Congress." The institution we once understood reasonably well underwent a major transformation in the 1970s. Johannes has successfully examined a small part, as have scholars such as Sinclair (majority leadership) and Kingdon (voting decisions). There remains a good deal to be done.

BURDETT A. LOOMIS

University of Kansas

Presidential Management of Science and Technology: The Johnson Presidency. By W. Henry Lambright. (Austin: University of Texas Press, 1985. Pp. xii + 224. $25.00.)

With the United States poised on the verge of making a multi-year, multi-billion dollar commitment to the Strategic Defense Initiative, this book on technoscience policy could not have arrived at a more propitious time. What this book has to tell us, however, is not particularly surprising. More than anything else, it is apparent that the scientific community (or "estate" as the author prefers to call it) acts much as any other interest group. Except, and this is a big exception, the scientific community has an institutionalized representative in the White House. One would think, given this sort of access, that the scientific community enjoys a unique advantage in Washington policy debates. This seems to be the case, within limits. Those limits come in the form of problems of implementation, particularly through the bureaucracy. Therefore, this book has both a new and old story to tell. We are reminded of the limitations inherent in the political process no matter how well connected the interest. At the same time, we have the opportunity to observe the institutionalization of policy making within a new and developing realm of governmental responsibility.

There are, of course, limits to the flexibility of case studies. But, in truth, this book is really not a case study. Students of the Johnson presidency will be disappointed to find that this is a book about technoscience policy that makes reference to the Johnson administration rather than the other way around. This, despite the fact, that the book is third in a series commissioned by the University of Texas Press to chronicle the administration of Lyndon Johnson.

For the specialist or the general reader interested in technoscience policy, however, this book provides a compelling if not conceptually complete framework for analysis. For one thing, it is not entirely clear what are the bounds of technoscience policy. Included in the 24 cases the author has chosen to examine, are issues as diverse as the Mohole Project (intended to get a core sample of the Earth's mantle) and the ABM. Obviously it would be difficult to suggest that many of the same principles apply to the consideration and implementation of these very different projects. Herein lies both the strength and weakness of this book.

To the extent that most technoscience issues fall in the range of low to mid-level policy making, this book gives us a unique perspective on the presidential administrative process. Presidential scholars have a tendency to choose for analysis cases that by virtue of being "major" policy issues are rather exceptional. Major policy issues are often kicked upstairs thereby circumventing the presidential bureaucracy and demanding relatively undivided presidential attention. These sorts of decisions, however, are the exception rather than the rule. The great strength of this book and what will be of most interest to presidential scholars is its focus on lower-level presidential decision making.

The great weakness of this book is that the author may not recognize the potential benefit for presidential studies of focusing on this lower-level process. So much has been written about the ABM and NASA that when the author delves into these programs little is provided that is new or interesting. After all, these are precisely the sort of issues that have been afforded book-length examination. It is when the author discusses the Mohole project or NIH funding or programs designed to predict earthquakes that we see a side of the presidency which is rarely exposed.

Given that there are characteristics special to the Johnson Administration, such as the penchant for a lack of follow through or the Vietnam War, there is still much to be learned from most of the cases outlined by the author. One only wishes that more attention had been paid to analysis of lower-level decision making. After all, once major issues are labeled national security concerns or special presidential projects, the scientific estate becomes just another lobby, and a minor one at that.

DANIEL PAUL FRANKLIN

Colgate University

Abortion and the Politics of Motherhood. By Kristen Luker. (Berkeley: University of California Press, 1984. Pp. viii + 324. $14.95, cloth; $7.95, paper.)

Since the abortion debate so often supersedes the other concerns of policymakers, candidates, and single-issue voters, one would expect to find many analyses of the politics surrounding this important issue. Unfortunately, however, most of the writing about abortion policies is intended to advance a particular point of view. Kristen Luker's book is a notable exception to that generalization. Luker says her purpose is the discovery of "how people come to differ in their feelings about the rightness or wrongness of abortion" (p. 3), and she presents her findings in a sympathetic but dispassionate manner.

Luker's book provides a history of the evolution of U.S. abortion policies, followed by a case study of the controversy in California over the last two decades. Her information was gathered from interest group literature and records, from newspaper accounts, and, especially, from lengthy interviews with more than 200 pro-choice and pro-life activists. Many quotations from the interviews are included in the book, thereby adding texture and feeling to the narrative.

Because Luker herself is a sociologist, it is not surprising that the strongest part of her book concerns the social background characteristics and "world views" of the activists. Although most activists are women, Luker describes them as "two very different constituencies of women, two groups that have different experiences in the world and different resources with which to confront it" (p. 8). By comparison with the pro-choice activists, the pro-life women are not only much more religious, but also have more children, are less likely to work outside the home, are less educated, and have lower income levels.

Given the major differences in their social circumstances, it follows that the world view of the pro-life activists should differ greatly from that of their pro-choice opponents. For both groups, the debate over abortion is "merely the tip of the iceberg" of diametrically opposite beliefs about women's roles, sexual behavior, and the nature of marriage and the family. Luker's comprehensive description of the two world views helps to explain why the abortion issue has generated so much emotionalism. She asserts that the debate is really "a referendum on the place and meaning of motherhood" (p. 193) and on "the meanings of *women's* lives" (p. 194). The activists on both sides have a personal stake in winning the abortion controversy, because a loss would represent "the very real devaluation of their lives and life resources" (p. 215).

Much of the information in this book is of special interest to political scientists. Luker describes the organizational frameworks and resources of pro-life and pro-choice groups, as well as their recruitment patterns, cohesion, and membership involvement. The tactics of grassroots mobilization and legislative lobbying are also explained, along with a critical evaluation of the strengths and weaknesses of the contending groups. Especially insightful is Luker's explanation of the "technology of telecommunications" (p. 219), which has enabled the pro-life women to be politically involved without leaving their homes.

Because the pro-life groups are more active, they receive more attention in Luker's analysis. She also chooses to confine her discussion to legislative lobbying, rather than deal, in addition, with interest group pressures on the bureaucracy and the courts. Furthermore, Luker's study concerns only state and local activists, so similar research still remains to be done at the national level.

Nonetheless, it is likely that Luker's book will become the best-known study of the abortion controversy, not only because of the dearth of direct competition, but also because of the intriguing insights it offers.

CONSTANCE EWING COOK

Albion College

Black American Politics: From the Washington Marches to Jesse Jackson. By Manning Marable. (London: Verso, 1985. Pp. ix + $27.50, cloth; $8.95 paper.)

This book examines the "theory and the historical practice of Black politics: the racial politics of class struggle, the pursuit of power in monopoly capitalist and colonial capitalist societies" (pp. 1–2). Although the author considers racism a manifestation of "false consciousness," he acknowledges that it is a powerful social force—a force that is logically and functionally related to class inequalities and oppression in the bourgeois democratic order.

Marable offers a provocative theory of black leadership. Black electoral politics is best understood as the "history of the class-conscious Black petty bourgeoisie seeking to influence the bourgeois democratic or colonial-capitalist state for its own purposes. Such political intervention may or may not serve the social class interest of the Black majority" (p. 174). Among the presumed reasons for this dereliction of leadership is the petty bourgeoisie's unwillingness to commit "class suicide," its propensity to be self-serving and self-interested, and its failure to undestand that racial equality cannot be achieved within the liberal democratic state. The most predictable characteristic of this class is its "political and ideological inconsistency" (p. 157).

This theory of black politics is applied to the analysis of the 1941, 1963, and 1983 marches on Washington, D.C.; the electoral politics of Chicago, including the 1983 election of Harold Washington as mayor; and the presidential campaign of Jesse Jackson. In each case, Marable is critical of the black middle-class leadership—especially the National Association for the Advancement of Colored People—for its role in dampening the militancy of these movements. That the Washington and Jackson campaigns produced advanced agendas is attributable in part to the initial reluctance of many main-stream black leaders to be identified with the candidates, thus leaving a vacuum into which left-progressive leadership moved. Jackson himself "may best be understood as an ideological weather vane, an ideological and changeable indicator of the mind and mood of Black America" (p. 266).

In Marable's view, the next stage in the black struggle should be a revolt against the Democratic party in order to force the petty bourgeois black leaders to "either commit class suicide or openly defend the status quo" (p. 189). This move would pave the way for "militant, uncompromising leaders [to emerge] from the oppressed social classes" (p. 189).

Given the legitimacy of liberal democracy in the United States, the low level of class consciousness, the extent and character of white racism, and the deficiencies of black leadership that Marable asserts, this reviewer cannot see as realistic the scenario for the socialist revolution he describes. Neo-Marxists, however, tend to see history as more teleological than the reviewer. To them, social movements are "pre-revolutionary modes of social class struggle which set the historical stage" for the social revolution (p. 78). Accordingly, the revolution will simply be the culmination of the many struggles—for example, the marches on Washington, the Jackson campaign—that precede it. Whether this logic salvages the argument remains to be seen.

It is difficult to do justice to this complex book in a brief review. Partly, this is because of the ambitious scope of the work; partly, it is because of the way it is organized and written. An example of the latter difficulty is that the chapters are too long (the first is 74 pages), and are broken internally only by Roman numerals. Moreover, the prose tends to be cumbersome and obscure. Marable uses statistics frequently, but includes no tables; all the numbers are spelled out in the text.

Still, because of its historical reach, I would recommend the book to anyone interested in black politics. My reservations about its theoretical underpinnings notwithstanding, I would especially recommend it to students of black political leadership.

TOBE JOHNSON

Morehouse College

Beyond Entitlement: The Social Obligations of Citizenship. By Lawrence M. Mead. (New York: The Free Press, 1986. Pp. xi + 318. $19.95.)

Early in this thoughtful, thoroughly researched, and fervently argued book, Lawrence Mead makes his argument clear:

> The thinking behind [welfare] policy in Washington has been too narrowly economic. The programs claim to serve equality, but essentially all they do is transfer benefits to recipients. As long as equality in America requires competence as well as income, programs must set standards as well as support people. The most vulnerable Americans need obligations, as much as rights, if they are to move as equals on the stage of American life. (p. 17)

Near the end of the book, Mead lists what "seem to be the main social obligations expected of adults in the United States today." His list begins with work in available jobs for heads of families, unless aged or disabled, and for other adult members of families that are needy. It goes on to include contributing all that one can to the support of one's family; fluency and literacy in English, whatever one's native tongue; learning enough in school to be employable; and law-abidingness, meaning both obedience to law and a more generalized respect for the rights of others.

Why is it that these main social obligations expected of the general run of adults are not, in Mead's view at least, expected of welfare clients? In a word, over-permissiveness. In a paragraph:

> New Deal policy had confined government's responsibility to providing certain new economic protections and opportunities; it was the responsibility of ordinary people to make good use of them. In the Great Society conception the public responsibility was potentially unlimited. Government could never say it had done enough for the poor, because the burden of change was not shared with them. That feeling helps to explain the restless innovation of Great Society policy. Planners had to keep coming up with new programs until they found something that "worked," that is, some benefit to which poverty responded without obligating the poor themselves. (p. 59)

Because the Great Society emphasized society's responsibility to the client rather than the client's equal responsibility to society, Mead tags the programs of the Great Society as the cause of today's welfare woes. He notes the sharp increase—from 63% to 91%—between 1967 and 1970 in the share of eligible female-headed families actually on welfare, and correctly concludes that "something dramatic happened to break down the stigma surrounding welfare." That "something" was a change in governmental attitude at federal and state levels from a mean-spirited reluctance to honor their own welfare statutes to an overdue willingness to abide by the spirit of the thirty-year-old law. Just so, yet the change is not something of which Mead approves, because "it can demoralize recipients" (p. 64).

Neither Mead's careful scholarship nor his deft presentation of the results of the negative income tax experiments, however, preclude arching an eyebrow over the apparent implication here that public benefit programs should be showpieces, that their merits are greater when they are passed over by the legally-eligible population. Can it be, for example, that civic obligation would best be served if an ever-larger number of taxpayers simply passed over the personal exemption that is explicitly and unequivocally provided for in the Internal Revenue Code, and by so doing significantly cut away at government tax expenditures? Should there be a massive campaign to stigmatize high-income persons eligible for social security who do not follow President Reagan's example and decline to file for benefits? The point need not be dwelt on. A government of laws demeans itself if it writes laws—whether restraints or benefits—and allows those who simply follow the law to be marked with a scarlet letter.

Welfare law, to be sure, becomes Mead's ultimate target; sensible restructuring of laws and regulations to obligate beneficiaries to take work more seriously emerges as the ultimate message. The book seemingly espouses welfare paternalism of a type practiced in Switzerland, most recently described by Ralph Segalman in the Winter 1986 issue of *The Public Interest.* If it begs the question of how government's willingness to take on the posture of enforcer of civic obligations is affected in the United States by race, class, and regional complexities, the sure-footed tone would encourage a belief that Mead does not necessarily shy away from that thicket. One hopes he may take it on as a later stage in the development of the argument.

Whatever one's reaction to the policy position, Mead's effort stands as a classy model of public policy analysis. *Beyond Entitlement* provides as good an overview of the literature on the politics of welfare as has appeared during the last 25 years. The author argues his position with passion and decency. In the process of so doing, he also explains a lot about the development of social policy.

GILBERT Y. STEINER

The Brookings Institution

Unnatural Monopolies: The Case for Deregulating Public Utilities. Edited by Robert W. Poole, Jr. (Lexington, MA: D.C. Heath & Company, 1985. Pp. xiv + 224. $25.00, cloth.)

This book is not—and does not purport to be—a well-balanced analysis of arguments for deregulating public utilities. The editor and the authors all agree that deregulation is desirable. From their perspective, the question is not whether to deregulate but how and when. In seven articles that focus primarily on telephones and cable television, they attempt to make a case for radical deregulation. Unfortunately, they do not succeed.

A key problem is that the authors produce little evidence to support the propositions that deregulation will ensure competition and that competition will promote efficiency. Instead, they rely heavily on theoretical arguments that will please those already enlisted in the cause but will not persuade more skeptical readers. For example, Cornell and Webbink denounce rate-of-return regulation as "an unacceptable fraud on the public." Utility regulators cannot hope to make sound judgments, they contend, because they depend exclusively on regulated firms for data. This assertion exaggerates the importance of raw data and ignores the growing capacity of public utility regulators for independent data analysis. Public utility commissions now have large professional staffs to analyze data and audit utility company books. Moreover, one-third of all public utility commission staff members have had some experience working with regulated firms, so that they are not easily hoodwinked. Ironically, the "revolving door" helps regulators to challenge utility company interpretations of raw data. In addition, regulators often benefit from submissions by consumer advocacy groups, some of whom present their own econometric models, forecasts, and rate schedules as alternatives to those advanced by utilities. These observations apply more to state and federal regulators than their local counterparts. Regrettably, the differing abilities of different levels of government to regulate in the public interest are never discussed.

Two of the authors do make a systematic effort to examine the consequences of direct competition as an alternative to monopoly. In an interesting piece on cable television, Hazlett cites data showing that cable television prices are no higher in cities where cable television companies compete directly for customers than in other cities. Without data on profits, it is difficult to know how long this will last. Also, without controls for population density, penetration rates, and other variables, it is difficult to draw inferences from these findings. Still, the article is worth reading. In another interesting chapter, Primeaux concludes that direct competition between electricity distribution firms results in lower average costs, at least in smaller cities. For reasons he explains, Primeaux limits his analysis to municipally-owned firms, although they account for a very small fraction of the total electricity output in the U.S. Unfortunately, municipally-owned utilities are not easily compared, because tax payments and tax-equivalent payments are handled differently in different cities. Moreover, Primeaux's data, from the 1960s, antedate a whole series of important changes in the economics and politics of public utilities. In particular, regulators in the 1970s took a variety of steps to improve the performance of utility monopolies (management audits, incentives to lower costs, etc.). Primeaux's data, nearly 20 years old, tell us nothing about whether these efforts have succeeded. Nevertheless, this article will be of interest to specialists in the field.

Readers who study public utilities will learn something from this book. There is useful information on historical trends and developments (Hazlett, Samuel, Mellor and Allen) and on new technologies and their implications (Samuel, Jackson). Several of the articles, though flawed, are provocative (Webbink and Cornell, Primeaux, Hazlett). Also, the book's single-minded enthusiasm for deregulation is tempered somewhat by brief critical comments following six of the seven chapters. Although the commentators seem sympathetic to deregulation, they do not hesitate to point out weaknesses in the authors' arguments. Moreover, their criticisms are typically concise and incisive.

Yet, these brief comments by discussants cannot compensate for other problems. The authors are not sufficiently sensitive to the complexities of regulation today—the multiple purposes of regulation, the different capacities of different levels of government, the potential for innovation within a regulatory framework, and the extent to which interest groups other than utilities make valuable contributions to the regulatory process. The recommendations are stronger than the evidence on which they are based, as several of the discussants note. Finally, this is a book that could have benefited from stronger editing. The editor's introduction is brief, and the reader must wade through some pretty difficult material with little help. There is no concluding chapter in a book that desperately needs one. In addition, one wonders why so much attention was paid to telephones and cable television (the focus of four of the seven articles), while other utility monopolies were ignored (natural gas companies, water and sewer utilities, refuse collection, the U.S. Postal Service). Is the case for deregulation weaker in these domains or was it difficult to find contributors on these topics? Without more information from the editor, it is difficult to know for sure.

In conclusion, this book is somewhat disappointing. It is narrow in its perspective and its scope. While specialists in the field may wish to read two or three of the articles, there is little to commend the book to a wider audience.

WILLIAM T. GORMLEY, JR.

University of Wisconsin
Madison

American Domestic Priorities: An Economic Appraisal Edited by John M. Quigley and Daniel L. Rubinfeld (Berkeley: University of California Press, 1985. Pp. xv + 398. $32.50, cloth; $9.95 paper.

The Reagan Administration's "New Federalism" proposals represent a major departure from the trend since the New Deal of active federal involvement in state and local policies. This collection of essays by 12 distinguished economists poses two interrelated questions about the New Federalism: What would the costs and benefits of transferring public functions from the federal government to the states? and, Is it possible to reduce the federal deficit through cutbacks in federal domestic programs?

Perhaps the most interesting feature of these essays is the general consensus that reducing federal control over domestic programs (with the exception of poverty and welfare) and increasing the control by states and localities could in many cases result in a more efficient allocation of resources. This conclusion rests on the argument, presented by Edward Gramlich, the "Public goods [are] to be provided by the jurisdiction covering the smallest area over which benefits are distributed—that way the public-goods efficiencies are maximized and the effect of taste differences is minimized." This conclusion is surprising since it comes from a group of economists who, in the past, often advocated a strong federal role in domestic policy. This endorsement of a more decentralized domestic policy approach is a measure of the New Federalism's success in transforming the terms of the policy debate.

Despite endorsing more decentralization, John Quigley, Daniel Rubinfeld, and the other contributors to this volume do not endorse the Reagan Administration's New Federalism proposals. Indeed, they persuasively argue that the New Federalism proposals were primarily designed to reduce federal spending by transferring costs of public functions from federal to state and local authorities, *not* by a desire to transfer power to the states. As Henry Aaron points out, the New Federalism was a "raw deal for the states." In the long run, they would lose a lot of federal money and take on new responsibilites. If the major purpose of the New Federalism was to transfer power to the states, the Reagan proposals could have sweetened the pot financially for the states.

In their concluding essay, Quigley and Rubinfeld assert that neither cutting federal domestic programs (other than Social Security) nor transferring control of these programs to the states offers much promise of substantially reducing the federal deficit. They advocate instead increasing federal tax revenues by disallowing federal deductions for mortgage interest payments and for state and local taxes, and by removing the federal tax exemption for obligations issued by state and local governments. They argue that this set of federal tax breaks disproportionately aids higher income households because it benefits homeowners and taxpayers who deduct. They also argue that these tax breaks will distort state and local spending decisions since some of the costs of these decisions will not be borne by local taxpayers.

Despite a general agreement that domestic policy making might benefit from a more decentralized control, the economists in this volume disagree considerably about particular fiscal issues or policy strategies. These disagreements will be informative and provocative for students of economic policy. Gramlich, for instance, questions economists' conventional wisdom that economic stabilization policies are best conducted at the federal level, and presents an ingenious case for subnational fiscal policy. Quigley and Rubinfeld disagree with Gramlich. The difference between these two positions rests on different assessments about the mobility of labor and goods across regions.

There is also disagreement over the proposal to eliminate deductions for state and local taxes. Gramlich, Aaron, Quigley, and Rubinfeld recommend eliminating these deductions for the equity and efficiency reasons discussed earlier. George Break, on the other hand, argues for federal deductibility on the grounds that people should be taxed on their discretionary income; that is, payment of state taxes reduces the income available to pay federal taxes. Break recommends changing the federal deduction for state taxes into an exemption so as to eliminate differences in tax treatment between those who deduct and those who do not deduct.

These essays are quite ambitious. The authors provide an overview of major policy areas (e.g., housing, environment, transportation) in which they review policies in the areas, assess the research evidence about the usefulness of various policy strategies, predict the results of funding cuts, and propose alternative policy strategies. A good example is Jose Gomez-Ibanez review of the federal role in transportation. Gomez presents a compelling case that the availability of discretionary mass-transit grants has resulted in the building of many new and probably unnecessary rail systems.

John Kain's chapter on black suburbanization in the eighties is excellent. Kain shows that there is still considerable segregation in both cities and suburbs and that this segregation is not primarily due to the low incomes of blacks. He notes a very hopeful new phenomenon. In an analysis of residence patterns in four metropolitan areas, he finds an important qualitative change between 1970 and 1980 in blacks' living arrangements. In 1970, many suburbs had less than five black households; by 1980, most suburban communities had more than five black households. Kain argues that federal support of civil rights and fair housing likely contributed to this trend and strongly endorses continued federal support for civil rights and fair housing.

In sum, this book on *American Domestic Priorities* provides a lively, informed, and solid discussion of whether the federal fiscal system ought to be restructured, and of a wide range of domestic policy issues. The essays in this book should prove a valued resource for teachers, policy researchers, and policy makers.

MARY CORCORAN

The University of Michigan

Neighborhood Organizations: Seeds of a New Urban Life. By Michael R. Williams. (Westport, CT: Greenwood Press, 1985. Pp. xiii + 278. $35.00.)

How realistic is it to expect that disadvantaged citizens can influence urban policy decision making at the local level? This is the central question that Michael Williams poses in *Neighborhood Organizations: Seeds of a New Urban Life*. Drawing upon the organizational behavior and citizen participation literature and his own experiences in neighborhood activism since the late 1960s, Williams offers an analysis of how neighborhood organizations can mediate the concerns of the disadvantaged vis-a-vis corporate and governmental influences in America's cities.

The notion of neighborhood organizations as mediating institutions was originally developed by Peter L. Berger and Richard J. Neuhaus in *To Empower People: The Role of Mediating Structures in Public Policy*. (Washington, DC: American Enterprise Institute, 1977). Williams extends their arguments by suggesting that advocacy-oriented neighborhood organizations can redefine the problems of the have-nots who reside in urban America. Yet Williams recognizes that neighborhood organizations need the financial support of a federal government committed to reinvestment if the disparities between the "haves and have-nots" are to be addressed.

The book is organized into four parts. Part 1 examines the setting for neighborhood organizations and competing conceptions of neighborhoods, while part 2 studies the internal dynamics of neighborhood organizations. Drawing upon concrete examples of neighborhood activism in the areas of energy, crime, and education policy, Williams argues in part 3 that the central task of neighborhood organizations "is the development of an advanced level of knowledge, participation skills, and organizational abilities within the community" (p. 147). It is in part 4 that Williams outlines his plan regarding how citizens can play the key role in urban service delivery. A useful concluding bibliographic essay summarizes the neighborhood organization literature discussed throughout the book.

While Williams offers a spirited argument on behalf of neighborhood organizations, he fails to address several key issues. Williams ignores the likelihood that neighborhood organizations increasingly will be competing against one another in a zero sum environment of fiscal retrenchment at the local level. Little is said regarding the impact of the Reagan administration's cutbacks of programs such as the Urban Development Action Grants (UDAG) and the Community Development Block Grants (CDBG) upon urban service delivery.

Williams does identify the key conflict between citizen participation and technical expertise that confronts community organizers at the local level, but he fails to offer persuasive suggestions regarding how citizens might be provided with the technical expertise needed to understand the intricacies of urban policy initiatives. It may be too much to expect that neighborhood leaders will be able both to mobilize and to educate the citizenry. Perhaps urban planners can play a valuable role here.

A central theme of the book is the importance of conflict between community group members and decision makers if the concerns of the "have-nots" are to be protected. Yet Williams does not adequately specify the kind of conflict that will rekindle "the fragile and abused ideal of democracy" (p. 25). He might also have outlined more clearly how this conflict will hold corporate elites and bureaucrats accountable to the citizenry.

The book does not clearly identify how citizens will be mobilized to participate in neighborhood organizations. For example, Williams ignores the fact that many urban neighborhoods are artificial boundaries created by urban planners. Citizens living in these artifically created neighborhoods may not have the sense of community that the author feels is necessary for active participation in neighborhood organizations. To suggest that local activists will mobilize large numbers of the disadvantaged residing in urban areas may be overly optimistic. Problems of time, lack of resources, and an absence of program information are barriers that must be overcome.

Despite these shortcomings, *Neighborhood Organizations: Seeds of a New Urban Life* should be required reading for students of urban policy, citizen participation, and organizational behavior. Williams has offered a useful contribution to the literature regarding the role of neighborhood organizations in a viable demoracy.

CRAIG A. RIMMERMAN

Hobart and William Smith Colleges

The Rise of Modern Judicial Review: From Constitutional Interpretation to Judge-Made Law. By Christopher Wolfe. (New York: Basic Books, Inc., 1986. Pp. xii + 392. $24.95.)

When policemen broke into his bedroom, one Rochin hastily swallowed a pill that lay on the bedside table. The cops dragged him out of bed, hustled him to the station, and forced a stomach pump into him. The disgorged pellet was the decisive evidence that led to his conviction on a narcotics charge. But in the end he was freed. Justice Frankfurter, for the U.S. Supreme Court, said that he had been deprived of his liberty without due process of law, because the conduct of the police "shocked the conscience." Frankfurter's frequent antagonist, Justice Black, agreed that Rochin had been unconstitutionally convicted, but for a different reason: Rochin, Black said, had been compelled to testify against himself, and such compulsion violated the Constitution. Black was scornful of Frankfurter's position. If the test of constitutionality, he said, was "the conscience," then the Court would decide cases on the basis not of the Constitution, but of the judges' own sensitivities, preferences, "consciences." The Court should not do this. In recent decades, the justices had struck down laws regulating business, ostensibly because such laws violated the due process clause, but really because they disapproved of such regulation. They had improperly exercised a legislative judgment. So Black argued—but was he not himself legislating, when he said that being subjected to a stomach pump was the same as being compelled to testify against ones self?

If Christopher Wolfe had dealt with the *Rochin* case, he would have argued that yes, both Frankfurter and Black were turning judicial review away from its proper function of interpreting and applying the Constitution. Instead of just stating and enforcing the fundamental law as propounded by the framers two centuries ago, the judges were making law in accordance with their own ideas of what the law and the Constitution should be. The author, rather disarmingly, suggests that his own preference, like that of Attorney General Meese, is for judicial review based on the intentions of the people who drafted and ratified the document. Stomach pumps were unknown in 1791, when the due process clause and the prohibition of compelled self-incrimination became constitutional clauses; ergo, neither clause could help Rochin in 1952.

However, Professor Wolfe does a remarkable job of muting his own predilection and presenting a clear, readable, and fair account of the development of judicial review. He divides the history of it into three periods: first, from *Marbury v. Madison* to 1890, nearly nine decades when, on the whole, the Supreme Court stuck to traditional concepts of interpretation (a glaring exception was the *Dred Scott* case); second, 1890–1937, the period when the justices (Holmes dissenting) substituted their own preference for laissez-faire economics for that of Congress and state legislatures, and so themselves became, in effect, legislators; and third, the modern era of "judicial activism," when the Court, while no longer finding that the regulation of business is made impossible by the due process clause, has elevated the First Amendment to a preferred place in the constitutional constellation, and in so doing has departed from the intentions of the framers. All of these chapters, plus critiques of the relevant ideas of, among others, Woodrow Wilson and Ronald Dworkin, are worth reading, for legal scholars and laymen alike. They well serve the author's stated purpose: "the education of the American people to the true character of modern judicial review."

At the end, Wolfe speaks yearningly of the Constitution not as a " 'living Constitution' that is wax in the hands of those who apply it, but the Constitution fairly interpreted to faithfully express the meaning it was given by its authors and understood by those who gave it authority by ratifying it." We may agree with him that the federal judiciary is not a democratic institution, and still ask if democracy would really fare better if the courts went back to "traditional interpretation." For example, the Sixth Amendment gave defendants in criminal cases the right to have "the Assistance of Counsel." The authors of that amendment meant only that if a defendant wanted a lawyer at his trial, he could retain one. The idea that the State should appoint counsel for an indigent defendant never occurred to them. Yet in 1963, in *Gideon v. Wainwright*, the Supreme Court held that the Constitution gave the penniless Gideon the right to have the State appoint a lawyer to represent him. Should this decision now be overruled, because the Court read into the Sixth Amendment something that had not been in its authors' minds? That is the kind of question that Professor Wolfe, quite properly, asks us to debate.

THOMAS H. ELIOT

Cambridge, Massachusetts

COMPARATIVE POLITICS

Redesigning the State: The Politics of Constitutional Change in Industrial Nations. Edited by Keith G. Banting and Richard Simeon. (Toronto and Buffalo: University of Toronto Press, 1984. Pp. xii + 257. $30.00, cloth; $14.50, paper.)

This volume is an outgrowth of papers presented at a conference held at the Institute of Intergovernmental Relations of Queen's University, Kingston, Ontario, in April 1982. The conference took place precisely when the final accord putting an end to some 50 years of constitutional stalemate in Canada was reached. Against this backdrop, the conference must have been an extraordinary intellectual experience. The goal of the conference was "to bring together the experience of constitutional change from different settings and to trace the complexities of constitution making in particular cases" (p. 3). The pursuit of this goal consists of nine readable and interesting essays on European and North American case studies and general interpretations.

In the introductory essay, Keith G. Banting and Richard Simeon concern themselves not with comparing different constitutional designs and the prospects for reiterating constitutional choice, but with processes of attempted or actual constitutional change. They discuss several distinctions: the meaning and significance of constitutions and constitutional alterations; the sources of demands for constitutional change; the issues and interests at stake; the mechanisms and processes of constitutional debate; and, finally, the outcomes and their consequences. These distinctions do not appear to be derived from any theoretical construct. Instead, they appear to be distillations of positions that emerge from the case studies.

The case studies cover different types of political systems. Gerhard Lehmbruch sketches the recent evolution of West German federalism. Richard Gunther examines the constituent process that took pace in Spain following the death of Franco, and suggests reasons why the institutional reforms that resulted may be viewed as among the most far-reaching ever attempted in a modern industrialized society. Maureen Covell details revisions of the Belgian unitary state, including the 1970 agreement to create regional institutions, as a response to a long-standing conflict between that country's two language groups. Alan C. Cairns surveys the Canadian constitutional development that led to the proclamation of the Canada Act in April 1982. James D. Kellas explores the referendums in Britain over issues such as devolution, the European Economic Community (EEC), and local government finance in order to place in sharp relief the challenge of constitutional change in a country with no fixed procedures for constitutional amendment. Stephen Schechter traces and explains the politics of recent constitutional amendments in the United States as part of the mobilization of successive generations. Finally, Teresa Rakowska-Harmstone assesses the prospects for fundamental changes in Eastern European nations, where the monopoly of power exercised by the Communist party is not subject to constitutional constraints. The major generalization that emerges appears to contradict somewhat the very title of the book: there are little or no prospects for redesigning the state. As Tocqueville noted so well in his work on France, serious limitations exist in the politics of constitutional change. Once a particular organizational arrangement is in place, it is exceedingly difficult to undertake reforms that will lead to the transformation of the system into a different system based upon a radically different design. The essays by Richard Gunther, James D. Kellas, and Teresa Rakowska-Harmstone are especially insightful in this regard.

It must be added, however, that the collection of essays introduces few issues, arguments, interpretations and methods of analysis that are not generally known to individuals working in the particular areas. I say this as a description, not as a criticism. In the main, the collection makes a convincing case for viewing constitutional decisions, as Daniel Elazar points out in the concluding essay, as the preeminent political issues a country faces—a truism that needs restating "after a generation of withdrawal on the part of many political scientists from consideration of all that is labelled 'constitutional' in the world of government and politics, on the grounds that such matters are merely formal and hence not 'real' " (p. 232). The concluding essay is, in fact, vintage Elazar, original and suggestive in the way it tries to pull together some of the interpretive threads presented in this volume. Unfortunately, it is not, perhaps because of limitations of space, a sustained discussion of constitutional models and paradigms, but, in Elazar's own words, "a modest first step in that direction" (p. 247).

The volume succeeds in overcoming the parochialism of much constitutional debate in Canada and elsewhere. It deserves attention from Canadianists and comparativists alike.

FILIPPO SABETTI

McGill University

West German Politics. By Lewis J. Edinger. (NY: Columbia University Press, 1986. Pp. 330. $30.00, cloth; $10.00, paper.)

Policy and Politics in the Federal Republic of Germany. Edited by Klaus von Beyme and Manfred G. Schmidt. (NY: St. Martin's Press, 1985. Pp. 239. $39.95.)

These two books are notable contributions to our understanding of contemporary West German politics. Edinger has written a general and largely introductory account of politics and public policy in the Federal Republic, while von Beyme and Schmidt have brought together a number of original essays on policy making by German political scientists. A common feature of both books is their emphasis on the legal, political, cultural, and socioeconomic determinants of German public policy. They focus on the present, but are mindful of the historical and international context in which German politics is played out. While both rely heavily on empirical findings, they nevertheless hazard fresh interpretations of old and new data.

Edinger's is the more comprehensive—and better written—of the two books. A long-time student of German politics, he presents his "fourth major effort in about three decades to interpret ongoing political developments in the German Federal Republic" (p. xiii). His purpose is to map the changes that have taken place in West German politics, particularly in the 1980s, and to extrapolate from these changes possible scenarios of the future. His net judgment on West Germany is not set forth in any one place, but it can be summarized as follows: The Basic Law and the institutions created by it, including the federal structure and the formal framework of party government, have matured beyond the "possibility of significant alterations" (p. 319); not only is there "broad elite support for [present] political arrangements" (p. 132), but also, "most West Germans accept the legitimacy of the prevailing patterns of political authority" (p. 94); nevertheless, the body politic is ill, and the illness, while not serious, needs to be monitored.

Edinger's diagnosis focuses partly on the state of the German public mind and partly on the pattern of German politics. We find, for example, that popular participation in politics is insubstantial; that Germans are afflicted with a "low sense of political involvement and efficacy" (pp. 97 and 148); that they lack cohesive community sentiment (pp. 100–103); that the "average person has little or no control over public policy" (p. 104); that pessimism about the future pervades the young, especially the better educated (p. 105); that political leaders are coopted from the top rather than produced from below; that "the stability of the West German party system is . . . open to question" (pp. 178–179); that there are "severe restrictions on the formation and survival of new parties" (p. 144); that policy making results largely from group negotiations or a kind of "functional representation" (p. 188) "marked by the corporatist vestiges of a preindustrial society" (p. 219); and that "West Germans still look beyond these groups to the authority of the state as the supreme guardian of law and order and ultimate arbiter of socioeconomic conflicts" (p. 92).

When the reader confronts such statements about Germany's contemporary condition, he is not always sure whether they should be taken as criticism, although in most instances Edinger appears to be underscoring defects in the body politic. Because this book is written at such a high level of generalization, the reader coming to German politics for the first time is unaware of certain historical details and institutional considerations that would help to explain broad generalizations about politics in Germany. When, for example, the author tells us that the "Basic Law does not explicitly recognize interest associations, but . . . grants all citizens the right to form . . . groups provided they are not 'directed against the concept of international understanding' " (p. 182), the reader cries out for further explanation. The author deliberately sacrifices concrete details about political and legal institutions in quest of more universal propositions about the "policy environment," "political orientations," "Politicization and participation," and "party politics," all for the purpose of setting the stage for the discussion of policy making and public policy in the book's last three chapters.

These concluding chapters, which make up almost one-third of the book, are an excellent discussion of the problems and consequences of German public policy. Richer in detail than the previous chapters, they not only identify the forces and elites responsible for certain policy decisions, but also furnish the reader with information on the basis of which interesting contrasts with the United States can be drawn in such subject areas as abortion, education, social welfare, internal security, foreign policy, labor-management relations, and antitrust policy.

Policy and Politics in the Federal Republic nicely complements Edinger's overview of German policy making. In addition to von Beyme's opening chapter—a "systematic introduction" to the comparative study of policy making—it includes insightful essays on budgetary, social welfare, health, labor, housing, environmental, energy, internal security, and military policy issues. These studies, like Edinger's work, avoid rigid monistic explanations of public policy, and with their emphasis upon policy formation and implementation, they turn away from the radical criticism and ideological passion that have marked policy analysis in Germany. Edinger's work, however, tends to picture German policymaking generally as a process of negotiation and accommodation (often "behind the scenes") among various elites. These studies, on the other hand, detect a variety of policymaking styles. "Broad generalizations about a national style of policy-making," writes von Beyme, "tend to obscure historical changes over time, variations between fields of policy and the differences between the roles of various parties" (p. 18). A major conclusion of several of the chapters is that political parties do matter and that policy-making styles change with the changing complexion of coalition governments. They also find that policymaking styles range from the highly bureaucratic approach to a "participatory policy style," depending on the nature of the particular policy area under study.

One might conclude by suggesting that these two books would serve nicely in any class in German politics oriented to the policy-making process. The American student would find the von Beyme–Schmidt volume the rougher going. The writing style is often convoluted (vintage academic German), typographical errors abound, some passages are inelegantly translated, and, as with any multi-authored volume, the various chapters are mixed in quality. In a few cases the reader is aggravated by political judgments having no basis in the evidence presented. On the whole, however, the essays are worthy of study. Equally valuable is the extensive bibliography on policy and policy analysis attached to each chapter, although most of the citations are to German language materials.

DONALD P. KOMMERS

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Political Culture and Communist Studies. Edited by Archie Brown. (Armonk, NY: M. E. Sharpe, Inc., 1985. Pp. xii + 211. $14.95, paper.)

The essays included in this collection discuss the various strengths and weaknesses of using the concept of political culture in studies of comparative communism. The unifying core of the book emerged from a panel organized by Archie Brown on political culture and communist studies for the Second World Congress for Soviet and East European Studies held in West Germany during October 1980. The chapters contributed by Mary McAuley, Gordon Skilling, and Stephen White began as papers presented at this meeting. Yet, the volume clearly is much more than "conference proceedings," inasmuch as it features new additional essays by Archie Brown, John Miller, and David W. Paul.

Archie Brown's introduction highlights the conceptual evolution of political culture in comparative communist studies and the diverse interpretations given to political culture in the field. In turn, Mary McAuley's essay, "Political Culture and Communist Politics: One Step Forward, Two Steps Back," serves as the touchstone for the other contributions, framing the debate over the application of political culture concepts in comparative communist studies. Specifically, she divides the work of "political culturalists" in the field into two camps; "subjectivists," like Stephen White, who study individual-level subjective perceptions of history, politics, attitudes, and values to examine how political knowledge affects behavior in specific historical groups and nations; and, "non-subjectivists," like Robert Tucker, who examine the political system as a complex of ideal and real cultural patterns and political behaviors within existing political structures.

McAuley is more critical of the subjectivist school of interpretation, using examples drawn from several analyses of comparative communism, and particularly Archie Brown's and Jack Grey's 1977 work, *Political Culture and Communist Systems.* McAuley expresses her doubts about how culture is defined, what data are used to compare cultures, why dubious inferences are drawn from past beliefs to present behavior, and how traditional attitudes are seen as steering present-day activity. Since political culturalists want to argue "that past culture is a crucial factor in forming today's culture" (p. 23), she believes that models of contemporary culture must be based on explanations that firmly define the pattern of traditional values, test whether or not present-day behavior can be explained in terms of contemporary interests, and, if it cannot, then turn to how traditional values, when transmitted, must account for actions today. McAuley concludes that political culturalists continue to define their concepts poorly and often confuse their terminologies, while failing to apply their frameworks with care and dedication.

John Miller's essay continues McAuley's theoretical questioning of the political culture approach. Despite the obvious utility of some of its constructs, he challenges the uncritical acceptance by many scholars of Marxism-Leninism's content and Communist socialization at face value, as well as the tendency to assume a "one-to-one correlation" between what the regime teaches and popular beliefs. Instead, he stresses the importance of looking for the "hidden curriculum" in communist ideology, or the ironic discontinuities in political acculturation and individual socialization that actually contradict official Marxism-Leninism. To reinforce his point, he argues that it is not the content of political beliefs, but their form, defined in terms of cohesiveness and adaptive survival, that political culturalists should examine more closely. By comparing relative success of party recruitment in Estonia, Latvia, Uzbekistan, and Kazakhstan, Miller suggests the relatively low party saturation levels in Estonia and Latvia compared with the relatively high levels in Kazakhstan are largely a function of Stalin's destruction of the Kazakh cultural elite in the 1930s. Whereas Estonian and Latvian political culture was transmitted through a broader base of more independent and literate households, the nomadic Kazakh culture rested on a much smaller base of cultural elites. Consequently, for the Kazakhs, "to destroy their elite was literally to decapitate society" (p. 57). After the collectivization campaigns, then, Kazakh political culture lacked cohesiveness and adaptability, making many of its members more susceptible to appeals from the party.

In addition to these more theoretical discussions by Brown, McAuley, and Miller of the concept of political culture, some contributors to the volume also try to vindicate the political culture approach as an analytical tool to account for political change and continuity in communist states. Addressing this theme in the USSR, Stephen White's essay compares prerevolutionary Russia's political culture with other European regimes, both authoritarian-traditional and liberal-democratic. He suggests that much of the Tsarist autocratic tradition has continued into the Communist era. Even so, the Soviet leadership's intention to construct socialism has transformed many political values, social rituals, and cultural attitudes within that autocratic tradition. "Soviet Political Culture Through Soviet Eyes," also looks at the role of political culture in the USSR, but does so by focusing on how Soviet politicos and social scientists use the concept in political discourse. Since Lenin initially used the term "political culture" in the revolutionary era, the notion frequently has circulated in declarations by modern Soviet leaders. It also has directed the inquiry of Soviet social scientists investigating political socialization and education in the USSR. Thus, Brown sees the concept of political culture assuming greater significance in the USSR in both official propaganda and scholarship, which also conveniently provides a common vocabulary and intellectual interest for Western Sovietologists concerned with political culture.

The theme of continuity and discontinuity between pre- and post-revolutionary political culture likewise underpins H. Gordon Skilling's and David W. Paul's essays on contemporary Czechoslovakia. Skilling's essay highlights the importance of political pluralism in Czech political culture under Hapsburg rule, the First Republic, and the Communist regime. While German, Slovak, and Czech nationalism frequently undermined pluralism before 1939, pluralist practices seem to have remained a permanent feature of Czech political life, and to a lesser extent, as Paul notes, of Slovak life, despite the antipluralist fascist and communist regimes of 1939–1945 and 1948 to the present. The limited restoration of pluralism in 1945–1948 and 1968–1969 illustrates how resilient Czechoslavakia's underlying pluralist political culture remained, at least up until the Husak regime. Still, Paul and Skilling conclude that outside forces and Czechoslavakia's international situation have been the basic constraint on Czech pluralism as well as the ultimate source of changes and continuities in Czech political culture.

Archie Brown's conclusion accents the important contributions that anthropological and social psychological research on culture can make to the study of political culture and communist studies. In particular, he supresses the importance of distinguishing between the "dominant" political culture, or the fundamental political beliefs and values of the majority of the people, and the "official" culture of the party leadership and its norms for behavior within the state and society. Defining and explaining the cross-national and longitudinal variations in these two contending political cultures among communist regimes, Brown suggests, is one of the most promising avenues for moving forward the study of political culture in communist systems. All of the essays in the collection are well worth reading. Yet, McAuley's vigorous critique of political culturalist analysis and Brown's equally spirited concluding defense of this approach are the main attractions, as they expertly elaborate the promises and pitfalls of using political culture concepts in comparative communist studies.

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Party Strategies in Britain: A Study of the 1984 European Elections. By David Butler and Paul Jowett. (New York: St. Martin's Press, 1985. Pp. 172. $25.00.)

British Democracy at the Crossroads: Voting and Party Competition in the 1980s. By Patrick Dunleavy and Christopher T. Husbands. (Winchester, MA: Allen and Unwin, Inc., 1985. Pp. 320. $25.00, cloth; $13.50, paper.)

Britain at the Polls, 1983: A Study of the General Election. Edited by Austin Ranney. (Durham, NC: Duke University Press/American Enterprise Institute, 1985. Pp. 227. $27.50, cloth; $13.75, paper.)

"Any national election," write Butler and Jowett, "faces party leaders and party organisations with a challenge." The same might be said for political scientists. Elections are supposed to be central to the operation of a liberal democracy, and political scientists feel compelled to study each one as it occurs. But this latest crop of books on the 1983 British general election and the 1984 election of the British members of the European Parliament raises questions about how political scientists should tackle this task.

There are two principal actors in an election—the electorate and those hoping to be elected. To the study of the behavior and attitudes of the former political scientists can bring one potentially powerful tool: the sample survey. But the electorate plays only a minor role in this collection. Ivor Crewe contributes a useful chapter to the Ranney collection in which he attempts to account for the substantial net shift of support from laborer to the SDP/Liberal Alliance between 1979 and 1983. Crewe's work is based upon a large quota survey of the electorate (4,141 respondents) conducted on polling day and the day before by the Gallup organization for the BBC. Dunleavy and Husbands were able to fund a smaller and rather limited quota survey (1,023 respondents), which was conducted within three weeks of polling day, but only three of their nine chapters are based on its findings.

It is the politicians who consume the most space in this collection. From the pages of Dunleavy and Husbands, and of Ranney, one can acquire a detailed knowledge of their plans, hopes, and activities before and during the 1983 general election, while Butler and Jowett perform a similar function for the 1984 European election. Yet rarely do any of them do much more than describe the events they are concerned with. While we may gain knowledge, we do not necessraily gain understanding.

One's sense of disappointment is greatest with the Dunleavy and Husbands volume. Their introduction suggests that they have two distinctive aims. The first is "to break out of the sterile compartmentalisation of professional political science" by bringing together the study of electoral behavior and political party behavior. The second is their presentation of their own "radical" model of electoral behavior and party competition, whose distinctive position is an emphasis upon the creation of new cleavages in the electorate through the expansion of the economic role of the state. The aims of the authors are at least clear.

But the execution is disappointing. For example, their two chapters on the events between 1979 and polling day 1983 are little more than informed descriptive accounts of the period. Only a small proportion of the book helps to advance the case for the radical model. Often, as in their discussion of the influence of the media upon electoral choice, their limited cross-sectional survey design does not give them the data they need to pursue their arguments adequately. The discussion of party competition and of electoral behavior is not brought together in a way that reveals original insights. Only in their discussion of the possible implications of future developments in the role of the state in the economy's consumption side does their approach enable them to make a distinctive contribution to our understanding.

In contrast, the other two volumes eschew any explicit or implicit theoretical approach to the study of party strategy. Both concentrate on the presentation of information. In this limited role both are reasonably successful. Butler and Jowett adopt a characteristically clear and readable style, although at times one feels this is achieved at the expense of providing the reader with further (useful?) information. Austin Ranney unfortunately begins his preface with a misstatement of the level of turnout (it was 72.7% rather than 76.3%), while a slightly more active editorship would have reduced the amount of repetition of central campaign events in more than one chapter. Anthony King's summary of the events and style of the first Thatcher administration, however, combines elegance and comprehensiveness, while Michael Pinto-Duschinsky's chapter on the Conservative campaign and Peter Kellner's on Labour's have clearly benefitted from their having been involved in their subject's central campaign.

Yet one is left wondering how these books contribute to the study of elections. Few generalizations are offered or tested that extend our understanding of the British political system or its political parties. While commentary is offered on the wisdom, rationality, or efficiency of a party's actions, the criteria by which these judgements are made are rarely made clear. As a consequence, their value is confined to professional Britain watchers who already have an understanding to test against the information they are offered. Their value to students must be limited.

The failure of any of these books successfully to apply a theoretical approach to the study of party strategy suggests that political scientists as yet lack sufficiently useful tools to make an original contribution to the analysis of the "high" politics of a modern day election. The challenge in writing books about particular elections is that they are data-driven rather than theory-driven. If they are to contribute to our theoretical and empirical understanding, they need not only to present the new data, but also to test it against our existing understanding. They should question and challenge as well as inform. Political scientists need a strategy as well as the politicians.

JOHN CURTICE

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African Independence: The First Twenty-Five Years. Edited by Gwendolen M. Carter and Patrick O'Meara. (Bloomington: Indiana University Press, 1985. Pp. xiii + 364, $29.95.)

This collection of essays by 13 prominent scholars of African politics is the product of a series of seminars and a conference organized by the editors in 1983. The stated purpose of these gatherings was to take stock of the first 25 years of African independence. Unfortunately, little attention was paid to the question of what significance the first quarter-century of independent rule is likely to have for the second. As a result, one has a sense of deja vu as one ploughs through this volume. The basic problems of the continent and their causes were identified long ago, and have been discussed extensively in the literature: the marginality of African states and the weakness of attendant political institutions; economic stagnation, especially in agriculture; the dependency of African states on the international capitalist system; the appeal of Afro-socialist and Afro-Marxist ideologies. All have been given deeper treatment elsewhere, in many instances by the contributors to this volume.

At a time when the political economies of most African states are in an advanced state of disrepair, it is disappointing that this book offers little in the way of a systematic treatment of the policies, processes, and alternative scenarios through which African states might have some prospect of climbing out of their present morass. The problem for this reader is, at base, the failure of social science, as practiced by leading Africanists, to come to grips with Africa's future in light of its past. Are we only to debate the future prescribed by the World Bank in the Berg Report and its sequels? One should hope for more.

African Independence also suffers from a malady of many edited collections—the lack of a common theme, and what appears to be a complete lack of any theoretical framework or set of basic questions around which the contributors were instructed to develop their presentations. Other than the focus supplied by the topic, the volume lacks coherence and structure. Particularly disappointing in this regard is the absence of an introductory essay by one or both of the editors. Given that this volume and the seminars and conference on which it is based were to honor the culmination of Gwendolen Carter's 10 years of contribution to the African Studies Program at Indiana, it is especially unfortunate that this volume is not graced by an exposition of her insights on the continent.

Notwithstanding the above, *African Independence* contains several well-written and interesting essays and one superb contribution that can, and do, stand on their own. In their essay on the marginality of African states, Robert Jackson and Carl Rosberg present a nice discussion of the forgotten distinction between juridical independence and sovereignty as conferred by other states of the international system versus "empirical independence" and sovereignty based on conditions present within the territories over which African states claim jurisdiction. Donald Rothchild's essay on state-ethnic relations contains a useful review and updating of the nature, significance, and future of clientelist politics on the continent. In attempting to set forth a model of hegemonial exchange and bargaining between central and local political forces, Rothchild's contribution is the only one explicitly concerned with the important issue of the politics of state-peasant relations. One wishes there had been more. A third useful contribution is the excellent review of Africa's agrarian malaise by Michael Lofchie. While largely derivative of earlier work, Lofchie's chapter is a must for any student in need of a one-shot briefing on the subject.

Perhaps the best contribution is John Saul's essay on ideology, particularly the long section on the rise and evolution of Afro-Marxism that includes a mini-case study of Mozambique. For those who have found the recent outpouring of Marxist writings on Africa to be frequently preoccupied with theoretical knitpicking, and silent on the question of how a transition to socialism might be accomplished, Saul's essay is a refreshing departure that should be read by both those who work within the Marxist paradigm and those who do not. Though Saul's presentation is devoid of any data on the significance of Marxist formulations of class analysis (especially class consciousness) on the thinking of Mozambican peasants, his discussion of the evolution of ideology by the top leadership of the Mozambican regime is first-rate.

African Independence is thus a diffuse work with a few bright spots that merit the attention of generalists who desire a review and update on the region, or by those who are particularly interested in the topics of the essays singled out for distinction above. Specialists on Africa, and would-be specialists, however, would do better to reread and reflect upon the major research monographs that made many of the contributors famous.

JOEL D. BARKAN

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Development and Decline: The Evolution of Sociopolitical Organization. Edited by Henri J. M. Claessen, Pieter de Velde, and M. Estellie Smith. (South Hadley, MA: Bergin and Garvey Publishers, Inc., 1985. Pp. xii + 369. $34.95.)

The first sentence of the preface states that "this volume grew out of a three-day conference on the evolution of sociopolitical organization, held during the First Intercongress of the International Union of Anthropological and Ethnological Sciences in Amsterdam, April 1981." Indeed. The book consists of 21 chapters, organized in three parts, contributed by 21 authors from nine countries. The bibliography contains about 800 items, from Engels and Harry Eckstein through Marx and Morgan to Orenstein and Organski, but with a heavy preponderance of a variety of anthropologists. The range of substantive material, adduced mainly for illustrative purposes, is enormous. Opening the book at random I find references to information theory, the Aztecs, Guinea Papuans, Nazi Germany, the Aleutian Islands, Samori's state and Kenedugu in West Africa, Clovis and Charlemagne, bi-facial hand-axes in the Middle Pleistocene, Hans Kelsen and the Homeric trial, and, as the Germans say, *"und und und. . ."*

The preface states that the aim of the volume is:

> to explain the evolution of sociopolitical forms. . . .The evolution of sociopolitical forms is a domain within the wider area of social evolution.
>
> Basic to this subfield is the concept of politics. *Many researchers have tried to define it. . . . (p.* ??)

Here I was tempted to close the book at random, but I was rewarded for not giving in to the temptation.

Part 1 is an "Evaluation of Evolutionism." Christian Guksch, in chapter 2, "The Conceptual Approach or Modeling Evolution," writes:

> I would now like to claim that evolutionary culturology is a subset of synergetics (adhering to synthesis as methodological thema) that tries to identify order parameters that cannot meaningfully be reduced, such as ideology, bureaucracy, interests, or even science as a conceptual behavior. (p. 21)

The reward is not yet.

In chapter 5, "Generalized Coercion and Inequality: The Basis of State Power in the Early Civilizations," Bruce G. Trigger informs us, surprisingly, that information theory suggests "the origin of the state can be linked more directly to political than economic factors" (p. 48).

In chapter 7, "Rank Society or Rank Societies: Processes, Stages, and Types of Evolution," Anatolii M. Khazanov conveys the following insights:

> The principle of limitation of leading positions is inseparable from the very essence of leadership and its functions. This rule is also operating in those societies in which different spheres of leadership and corresponding positions are separated. Not only in the United States or France can just one of the presidential aspirants be elected. . . . (p. 83)
>
> The curious and unique peculiarity of the human mind is to conceive of the past in terms of the present. (p. 94)

Not yet.

Part 2 deals with "The Evolution of Sociopolitical Organization," and Part 3 is entitled "Toward a Model of Social Evolution." There are some nuggets to be found here for our profession. Ronald Cohen writes in chapter 18, "Warfare and State Formation," in a section on "The Origins of the Military":

> Warfare conditioned the rise of states where none previously existed. Fine, but what about its consequences as an aspect of state policy? Political scientists are deeply involved in the causes of war; anthropologists mirror this interest, although they also look at effects—up to a point. (p. 282)

The model of social evolution toward which the co-authors are moving is not clear to me. My difficulties arise both from their practice of writing highly condensed papers for learned journals, evidenced by the bibliography, and from my impatience with efforts to rediscover the (re)invention of the wheel as both artifact and metaphor. No, I don't think Aristotle, who is not cited, said it all. And yes, I have gone through the (re)inventing phase myself (see "Comparative Politics: A Comprehensive Approach," this *Review*, 56(1962): 577–95). However, editors should be able to put the contributions of so many bright and innovative scholars into a clearer, more pointed framework without being Procrustean about it.

I have written this review on the assumption, for which I lack empirical evidence, that anthropologists have a sense of humor that includes, where appropriate, *Galgenhumor.* I came away from reading the book confirmed in my old convictions about the primacy of politics and its study. If there are indeed "wider areas," "domains," and "subfields," and if there is really something called "culturology," I know which is the widest. I trust my readers do, too.

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Conflict and Violence in Singapore and Malaysia, 1945–1983. By Richard Clutterbuck. (Boulder, CO: Westview Press, 1985. Pp. 398. $24.00.)

Richard Clutterbuck's (Major General retired, British Army) most recent book updates his 1973 study, *Riot and Revolution In Singapore and Malaya, 1945–1963* (London: Faber and Faber Ltd.). The six volumes he has written in the interim on political violence worldwide have in no way altered his interpretation of the Singapore/Malay(si)a case. That is, parts 1 and 2, "Urban Revolution—Singapore" and "Rural Guerrilla Revolution–Malaya," are identical, down to the pagination, with the original study. Part 3 is new and covers the 1963–1983 period in the same chronological and descriptive style. The book recounts the history of modern Singapore and Malaysia in terms of their successful countering of threats to the emergence of quasi-democratic, pro-Western regimes, namely, labor and Chinese-school unrest in colonial Singapore; the 1948–1960 "emergency" against Malayan Communist Party (MCP) guerrillas; Indonesian confrontation of Malaysia in the early 1960s; and recurrent communal violence culminating in the 1969 Malay-Chinese clashes and the temporary suspension of parliamentary government in Malaysia.

The distinctive flavor of Clutterbuck's account reflects his experiences as a British military officer and Malaysian civil servant fighting Communist insurgency and helping to manage the transition to independence. His detailing of antiguerrilla campaigns, the recruitment of undercover agents, and sketches of key personalities provide information not readily available elsewhere. By the same token, the book has a pro-government slant (for a recent marxist analysis see Hua Wu Yin, *Class and Communalism in Malaysia: Politics in a Dependent Capitalist State*, London: Miriam Books, 1983), is of little analytic interest (compare, for example, Lucian W. Pye's *Guerrilla Communism in Malaya*, Princeton: Princeton University Press, 1956), and does not attain the imaginative syntheses of primary sources characterized by the best work in the field (for example, William R. Roff's *The Origins of Malay Nationalism* New Haven: Yale University Press, 1967, or Michael R. Stenson's *Industrial Conflict in Malaya*, London: Oxford University Press, 1970.).

Clutterbuck's basic conclusion is that constituted authority—particularly as embodied in the legal system, the police, and the military—must remain constantly alert to the dangers of overreaction to provocation. The rule of law, freedom of the press, and individual liberties should be maintained to the greatest degree consistent with meeting the challenge of those who do not accept the democratic rules of the game. Within this strategic framework, Clutterbuck focuses on such topics as the gathering of intelligence, the use of the military for development projects, and measures to assure the safety of government officials. These and other counterinsurgency principles derive from conflicts rooted in the decolonization process under the influence of what Clutterbuck calls Leninist (in urban Singapore) and Maoist (in rural Malaysia) models. Their broader relevance is problematic, as suggested by the historically overlapping, somewhat similar Vietnam experience.

In *The Long Long War: Counterinsurgency in Malaya and Vietnam* (New York: Praeger, 1966), Clutterbuck argued that Vietnam could survive as a non-Communist nation if the lessons of Malaya were properly understood and applied. Yet the Malayan Communist Party never managed to make inroads into the conservative Malay community. In Vietnam, no equivalent barrier existed. The critical importance of context is vividly seen in the effort to reproduce Malaya's New Village program in Vietnam's strategic hamlet program.

In Malaya about half a million Chinese, forced by war and economic collapse into squatting on the fringes of the jungle and comprising the Maoist sea in which the guerrillas swam, were resettled into villages where they could be protected and controlled. Clutterbuck adduces a number of cogent tactical considerations that explain why Malaya's success in this endeavor could not be replicated in Vietnam, but surely the basic reason, not subject to human will or judgment, was that the uprooted were not landless immigrants but peasants linked to their land through age-old, sacred bonds.

Part 3 of the book further underscores my claim. Over the last two decades Singapore and Malaysia have become world-class successes in terms of economic development and political stability. Lee Kuan Yew, whose political savvy and toughness was instrumental in the defeat of communism, continues as Singapore's prime minister more than 20 years after independence. In Malaysia, the same communal coalition that gained independence remains at the core of the ruling National Front. The current prime minister, like all of his predecessors, rose through the ranks of the dominant Malay party. Concurrently—and it is difficult not to presume a causal link—the MCP has been reduced to an isolated band on the Thai-Malaysian border, history appears to have bypassed Singapore leftists, and no serious communal clashes have occurred since 1969.

Clutterbuck concludes his book with an assessment of the prospects of Singapore and Malaysia circa the early 1980s. Here, as throughout, his discussion is a balanced presentation worth reading, albeit couched in an "establishment" framework and devoid of analytical innovation, provocative insight, or novel interpretation.

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Concorde and Dissent: Explaining High Technology Project Failures in Britain and France. By Elliot J. Feldman. (New York: Cambridge University Press, 1985. Pp. xvii + 201. $34.50.)

There is no dearth of comparative studies of European public policy making, but *Concorde and Dissent* is an interesting and welcome addition to the literature. Elliot Feldman has selected two policy initiatives in order to analyze the differences and similarities in French and British experiences with civil aviation. The author has a very specific goal in mind: to dispel stereotyped notions that French planning is superior to British pluralist decision making. He challenges the frequently voiced opinion that the cultural backgrounds of French and British decision-making elites differ. The first case study compares the outcome of the debates about the need for a "third international airport" in London and Paris; the second part is a detailed case study of the Concorde project. These empirical illustrations show that neither British ineptness nor French rational planning account for the construction of Concorde, and that decision makers held similar values and priorities during the debates on a third airport.

Paris, however, obtained its famous Charles de Gaulle facility, whereas the British could not decide upon the location and size of their planned third international airport. Superficially, therefore, the French were more successful in realizing their stated objectives than the British. Yet a closer look reveals a somewhat different picture. Even by the standards of the French planning agency, de Gaulle airport was an ill-conceived, seriously flawed white elephant. It exacted enormous social and economic costs, and the airport "is in the 1980s a monument to technical hubris and uncoordinated central planning" (p. 19). Ironically, the failure of the British to proceed with the building of a third international airport spared "the public most social cost and considerable capital investment" (p. 147). The British simply expanded the already existing infrastructure, which was more than capable of meeting the increase in air traffic.

Cultural similarities among British and French officials and politicians are revealed in the Concorde case study. Concorde was a truly joint Anglo-French project and a spectacular commercial failure. Beset by financial and technological problems, the French and British cooperated until the bitter end to complete the enterprise. As Feldman argues, Concorde cannot be understood by pointing to the history of French planning and British stagnation. In fact, both countries succumbed to very similar pressures. Labor unions strenuously objected to the discontinuation of Concorde, on the grounds that it provided much-needed jobs.

Feldman does not deny that British and French policy processes and organizational structures differ. In France, the technocracy has far greater autonomy than in Britain. The different outcomes in the airport debates reflect this divergence. As parliamentary democracies, both countries coped with political instability and frequent rotation of appointed officials. In Britain especially, government has changed color numerous times over the last two decades. However, the frequent changes in political leadership had a different impact upon the policy process in the two countries. Since bureaucracies have far greater independence in France, the involved state agencies could proceed with the plans regardless of the political change. By comparison, the formation of a new British government was accompanied by institutional reorganization. The British difficulty in accomplishing long-term goals derived from "inadequate continuity in political construction and oversight" (p. 156).

This is a well-written and carefully crafted argument. If fault must be found, it is the absence of an analytical or schematic presentation of the differences and similarities in the two systems. In a methodological appendix, Feldman briefly touches upon the structural similarities in different political systems; but he never delineates analytically the ways in which the political systems converge more than diverge, which is, after all, the crux of his thesis. Nevertheless, this informative study is a good starting point for further comparative investigations, and it is just plain fun to read.

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The State of the Police. By Phil Scraton. (Dover, NH: Pluto Press, 1985. Pp. viii + 171. $7.50, paper.)

Policing Industrial Disputes: 1893 to 1985. By Roger Geary. (New York: Cambridge University Press, 1985. Pp. vii + 171. $34.50.)

The Politics of the Police. By Robert Reiner. (New York: St. Martin's Press, 1985. Pp. xiii + 258. $29.95.)

In democratic societies the public delegates to the police the legitimate use of force or the threat of force to assure compliance and to accomplish their mandate of law enforcement and maintenance of order. Because of the delicate nature of this task, controls are imposed upon the police in order to assure that they do not transgress the boundaries of their mandate and in turn violate principles that bind a democratic society together. The controls include legal constraints, administrative constraints, and political constraints. The British police have long been supposed to have subscribed to all three constraints. In the course of the last decade, however, that position has come under the careful scrutiny of several scholars and has been found wanting. What prompted this interest has been the role the police have played recently in the quelling of labor unrest and urban disorder. The three books cited above represent some of the most recent critical analysis from this revisionist school.

In *The State of the Police*, Phil Scraton offers a left-of-center sociopolitical explanation for the growth of police autonomy. He develops two theses. One is designed to call into question the myth that police serve as neutral arbiters in social conflicts. This view is frequently expressed in the popular literature on the British police, and is often articulated by senior police officials. Scraton argues that the police have not been neutral in either their order maintenance or law enforcement responsibilities. He contends that they have always represented middle- and upper-class interests, and that they have never gained the support or possessed the consent of the working class. The other thesis, which is the principal focus of the book, presents a persuasive argument that the police have become an organization that is beyond the normal constraints of democratic society. Scraton offered several case studies to illustrate law enforcement's disregard for the law. In such instances, neither judicial nor organizational constraints served to check such behavior; in fact, it was frequently condoned. In those cases where some blame was forthcoming, it was directed at individual officers rather than at the attitudes and policies ingrained in the organization as a whole.

What proves most distressing to Scraton is the total lack of political constraint on the police. The British have long prided themselves on having local police committees that are supposed to consult with the chief constable. Scraton, points out, however, that there have been several instances in which these committees have either deferred all policy decisions to the chiefs or have been thwarted by the chiefs in their attempts to demand greater accountability. The result of all of this has been a growth in the centralization of the police system that has all the characteristics of a national police force. Scraton does not offer much hope for containing this growth in independence, which has been frequently justified in the name of administrative efficiency. The only course of action presently available to the citizen is the careful monitoring of police activities. This tactic may pressure the police into recognizing the legitimate role that political constraints have within the democratic process.

While information about industrial conflicts and the police response is not the principal source of interest for Scraton, it becomes the focus of Roger Geary's efforts in *Policing Industrial Disputes: 1893 to 1985*. This is a detailed and judicious historical assessment of the changing nature of the relationship between strikers and the police. Geary suggests this relationship has been marked by several distinct phases. Three critical variables—the tactics of the strikers, the policy of the police, and the role of the central government—are essential to understanding this transformation in attitudes and behavior.

Before the turn of the century, strikers often escalated their protest by destroying their employers' property and by violently attacking those who were sent to restore the peace. Throughout the twentieth century the tactics of the strikers changed to obstruction of the employer's operation. This led to a considerable reduction in violence that Geary characterizes as pushing and shoving between the strikers and police. Not only have the police tactics changed, but the nature of the police force has also been altered considerably. In the nineteenth century the army often assumed a major role in maintaining order at industrial disputes, because the manpower of the local police forces was inadequate. The arrival of the army usually prompted the strikers to respond with violence, which led at times to shots being fired by the military. In the twentieth century industrial disputes became the sole responsibility of the police. In light of the change in the strikers' behavior, the police responded in kind, developing several tactics designed to discourage violent clashes, such as serving as a liaison between the employer and the union, limiting the number of police at picket lines, explaining to the pickets what they could and could not do, and establishing a friendly rapport with the strikers. Finally, the central government significantly changed its role in industrial disputes. In the nineteenth century its role was negligible. Local authorities determined the need for military assistance and directed their mobilization and deployment. In the twentieth century the central government assumed the initiative by centralizing the method of gathering information about strikes and overseeing the police response to them.

Like Scraton, Geary ends his study on a somber note, with a consideration of the level of violence that occurred during the miners' strike of 1984. He attributes this in part to a change in the behavior of the strikers. The miners were not united on the decision to strike, and the militant majority on the miners' executive committee was no longer committed to seeking peaceful reform. Geary is particularly concerned with police tactics and their apparent lack of restraint in dealing with the situation. After the 1981 urban riots the police established a tougher policy and more forceful tactics in handling public order disputes. When the miners struck, the police employed those hardline tactics and violence escalated. They did not differentiate between an industrial dispute and urban disorder. Geary concludes by calling upon the police to make that distinction, and to establish tactics that employ greater restraint.

Because of the nature of the police response to the 1984 miners' strike and of the tactics employed during the urban riots of 1981, the police and their policies have been the subject of a good deal of partisan political debate. On the one hand, the right has blindly defended the police response as an expression of their political "law and order" platform. The left, on the other hand, has perceived the tactics as an illustration of a coercive strategy designed to mitigate any attempt to secure democratic control. In *The Politics of The Police,* Robert Reiner offers an insightful analysis of the flaws found in both positions. Following an extensive review of the historical and social science literature, he contends that the police are at a critical juncture in their evolution. What makes this period so significant is the fact that the police and their mission are on the verge of becoming thoroughly politicized in a partisan sense.

Reiner points out that the police are partially to blame for this development. Speeches by some Chief Constables and the Police Federation's "law and order" campaign that was designed to influence the 1979 election are cited as marked departures from law enforcement's noninvolvement in politics. Reiner's most telling arguments are directed at the myths espoused by the political right and left. The right has portrayed the police as the panacea for effective crime control. Reiner employs several British and American studies to prove that most police work does not involve law enforcement. Social service, and to a greater extent, order maintenance are the distinctive characteristics of what police do. The left, for its part, has characterized the police as an oppressive political force. Reiner admits that any relationship that includes a power dimension is by its very nature political, but he contends that the real debate among scholars has centered on the degree to which the police have been involved in partisan politics. The police have been adamant in denying the existence of partisan politics within the forces, and Reiner offers a qualified endorsement of that position. He suggests that the history of the police from 1829 through the 1960s is in part an illustration of law enforcement's efforts to depoliticize its role in society. He also points out that a distinction must be drawn between partisanship of intent and partisanship of impact. In a society that is unequal because of class, law enforcement may well be partisan in its impact, but, he argues, that does not mean it is partisan in its intent.

Reiner contends that both ends of the political spectrum must approach police reform with a proper understanding of police culture and practices. Presently, both the left and the right maintain perspectives that are flawed in historical fact and sociological insight, and this has led both to offer simplistic views on the nature of reform. He supports much of the reform agenda spelled out in Lord Scarman's report, because it is essentially designed to depoliticize the police and their policies.

Those who are interested in comparative police systems will do well to read these books. They should also be required reading for students of British politics. Much has been written about the socioeconomic travails of Britain, and these works offer another insight into the nature of that dilemma from the perspective of the need to maintain order within both society and the police system.

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A Village Without Solidarity: Polish Peasants in Years of Crisis. By C. M. Hann. (New Haven: Yale University Press, 1985. Pp. ix + 208. $15.95.)

Poland After Solidarity: Social Movements Versus the State. Edited by Bronislaw Misztal. (New Brunswick, NJ: Transaction Books, 1985. Pp. xi + 167. $19.95.)

Authors writing in recent years on Poland have found it virtually obligatory to include he noun or name "Solidarity" in the title of books or articles. It is assumed—probably by publishers more than authors—that this constitutes a good marketing technique and will encourage greater readership. The books by Hann and Misztal are the most recent illustrations of this phenomenon. What is really ironic, in their cases, is that Solidarity figures very little in their analyses. This may represent both a strength and a weakness.

Around 1979, before the onset of the "Polish August," Hann first took up residence in the remote Polish village of Wislok, located in the deslote Bieszczady hills of southeast Poland.

He had earlier completed an ethonographic work on an "atypical" Hungarian village, and he sought to examine further the impact of the socialist system on rural life in general, and on the evolution of a new community in particular. Wislok had been left destitute by the Polish militia in 1947 during "Operation Vistula"—the campaign to eradicate Ukrainian "brigandry" and produce an increasingly socially homogeneous postwar Poland. The Poles subsequently transplanted to the area were faced with basic community-building tasks. Futher, as Hann incisively argues, colonists from the Central European plain were singularly ill-suited to conduct agriculture in a mountainous area. The study provides an interesting account of the economic progress made by the postwar Polish settlers, and their often tense relations with the small group of Ukrainians and *Rusnaks* (Ruthenians) who had managed to survive in these lands.

A central thesis of the author is that the system of private agriculture has brought ruin to Poland. The peasantry, whose income is derived exclusively from the land, is inevitably being replaced by commercial farmers or by worker-peasants. But in insisting, 40 years ago, on the maintenance of individual farms, peasants essentially traded off indispensable state assistance (machinery, fertilizer, investment) for largely irrelevant legal deeds. It is to Hann's credit that he does not belabor this argument, for in the course of his analysis little evidence is marshalled to support the view that collectivization is a preferable form of agriculture system. If state intervention in agriculture is, on balance, helpful to the intermediate-sized family farm and to the specialist farmer, the graphic presentation of forms of "doing business' in the public and cooperative sectors only sheds further light on the emergence of "Individual Farmers' Solidarity" and the determination of all farmers to evade state takeover of land (now improbable following the constitutional enshrinement of the principle of private agriculture.)

For the student of Poland who is shell shocked by the spate and uneveness of recent literature on the country, *Village Without Solidarity* is a soothing experience. In an unpretentious yet forceful way we learn of the family system, community bonds, patterns of religous worship, and subjects of gossip in Wislok—the "atypical" Polish village. The book is full of data on the more obscure and the more trivial aspects of Polish society—for example, the survival of a Greek Catholic (Uniate) church in the area (the religion was officially banned in 1946), or the fact that the number of children living in the countryside remains 700,000 more than the number in urban centers (n. 1, p. 199). Such crucial information is regularly overlooked by sociologists and political scientists. If Solidarity is only of marginal concern to the author, and if his conclusion seems aimed more at the handful of Polish social scientists working in this field than at us, these tendencies only add to the charm and authenticity of the author's monograph.

It is embarrassing for political scientists when an anthropologist reveals greater theoretical and empirical aptitude in examining a highly politicized society, such as Poland has been in the 1980s. Misztal and his contributors are not all political scientists to be sure. They are, by and large, sociologists concerned with political phenomena; put another way, there are steeped in the specifically socialist East European tradition of plucking just the sociological feathers from the polity. Oftentimes this proves to be an irritating approach, for it absolves political leaders and political institutions of their obvious faults. The reason we learn nothing about Solidarity from this book is its abstruse and overindulgent theorization. Thus we find two different periodizations of the Solidarity movement in the very first two chapters. Contributors pay lip service to the concepts of *state* and *civil society* (presumably the volume's adhesive substance), but they are never defined (other than in a largely irrelevant way by Markus in chapter 4), and *rulers* and *ruled* would have proved just as adequate terms anyway. From the acknowledgements the reader ought to have been forewarned something was amiss in this study: four of the six speakers (the most senior) invited to the conference on this subject did not contribute to the edited volume. A couple of the substitutes (Lewis and Szajkowski) rehashed impressive research published previously. While the contributors consist of an otherwise distinguished group of East European specialists, the chemistry was lacking here. Misztal's concluding chapter is a critique of (mostly) American literature on—just for starters—the state, social movements, urban sociology, and political economy. He is very unkind to it all, no more so than when evaluating the innovative ideas of urban theorists. The proofreading is catastrophic, ending up, fittingly, with an author named "Croom Helm" listed in references to Misztal's final chapter.

Political scientists and political sociologists ought, perhaps, to take a respite form theorizing about Solidarity, and to resume empirical research, which they appear to have suspended with material law in 1982. Let the Polish opposition movement work on model building: whatever one's attitudes to them, Kuron and Michnik are brilliant threoreticians with few equals among Western scholars. Finally, one longs to learn more about Solidarity, but perhaps from memoirs written by those who were involved. In this respect—unfortunately for Solidarity sympathisers—the government's former "contact man" with the union, Rakowski, has been most prolific and revealing.

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Small States in World Markets: Industrial Policy in Europe. By Peter Katzenstein. (Ithaca, NY and London: Cornell University Press, 1985. Pp. 268. $29.95, cloth; $10.95, paper.)

The volume under review is one part of a twin; both volumes are concerned with the political economy of "small states." The present study deals with the industrial policy of the small European states as a group, while the companion volume, *Corporatism and Change: Austria, Switzerland, and the Politics of Industry,* examines two closely related, yet contrasting national economies, Austria and Switzerland (Ithaca, NY and London: Cornell University Press, 1984). Yet the two can be read and understood independently of each other, and both deserve the serious attention of a variety of scholars, for they deal with issues crucial to modern industrial states.

Katzenstein's study of small state industrial policy is exemplary in several ways. First, Katzenstein engages in comparative analysis at the national aggregate level. In doing so, he uses a sizeable body of number without ever descending into any of the inanities of that methodology. The author has a thorough grasp of the national and social contexts from which these data arise, and of what they mean in their respective contexts.

Second, the book provides testimony to the effective use of history by a social scientist: Katzenstein does more than just embroider an otherwise bare cross-sectional contemporary study. History is used here in broad strokes, yet carefully, to document both the commonalities that characterize the political economies of the small states of Europe and what distinguishes them from each other. The history chapter does not appear at the outset, where it is usually placed, only to be put aside in any subsequent analysis. History comes *after* used to help explicate the origins of the various small state features already examined fully in their contemporary setting. That, it seems to me, is the proper use of history by a social scientist.

Third, students of corporatism can be grateful to Katzenstein for aiding them in their defense against the strong tendency in the discipline to dismiss their concerns because *(a)* "there ain't no such thing" in the U.S.; *(b)* the concept is nothing more than typical European myth making; and *(c)* we know that all corporatism ever was, or can be, is a "fig leaf for absolutism," as the late Herman Finer pointed out correctly in the case of Italy. Katzenstein demonstrates that corporatism is alive and well and living in a number of European places, that it refers to concrete social arrangements, and that it fully compatible with well-functioning democratic institutions.

The structure of the argument, and of the book as a whole, is simple, yet persuasive and exceedingly well developed. What distinguishes the small states of Europe from the large ones, according to Katzenstein, is the manner in which they combine a liberal external economic policy that keeps the country open to the forces of the international economy with a policy of domestic compensation through which the country's economy is buffered, to some extent, from the consequences of liberal openness. However, this small-state industrial policy does not become a straight jacket, as is the case in some large states; nor do the small states neglect industrial policy altogether, as do some other large states. At the same time, it is demonstrated that not all small states have settled on the same mix of international liberalization and domestic compensation in pursuing their industrial policy. Some pursue a much more active industrial policy than others. It is also worth noting that the particular small-state industrial policy Katzenstein specifies is not presented as resting entirely on the logic of size—that is, size is not seen as exclusively determining industrial policy. The historical foundations for the various types of small-state industrial policies enter the explanation in a significant way.

After laying out, in chapter 2, the three principal elements of the analysis—international liberalization, domestic compensation, and flexible response—and after demonstrating interstate differences using Austria and Switzerland as case studies, Katzenstein devotes chapter 3 to a presentation and analysis of corporatism, the particular arrangement that makes possible the domestic compensation for the international liberalization policies pursued by small states. Eschewing rigid ideal-typical constructs, such as "societal corporatism" and "state corporatism," he begins by observing that what he comes to characterize as corporatism is essentially a democratic corporatism. No extended arguments are necessary to so characterize the polities and societies of the Scandinavian countries, Benelux, Austria, and Switzerland. The small-state democratic corporatism that makes possible the mechanism of domestic compensation has three defining characteristics: an ideology of social partnership, a centralized and concentrated system of economic interest groups, and an uninterrupted process of bargaining among all the major political actors. Corporatism is also a result of the distinctive party systems of the small European states, in which the parties of the right are divided and the systems of proportional representation favors coalition government.

The European democratic corporatism is not monolithic, however. Katzenstein has been able to specify two major types: "liberal" corporatism, characterized by strong, centralized business organizations and weak, decentralized labor unions, as found in Switzerland, the Netherlands, and Belgium; and "social" corporatism, characterized by strong labor and weak business groups, as found in Austria, Norway, and Denmark. Sweden is said to constitute a mixed type. Chapter 3 then proceeds to examine in detail the workings of these various manifestations of democratic corporatism, and the analysis succeeds in demonstrating the success of the author in specifying a range of similarities and differences between the small states and the large ones (France, Great Britain, Federal Republic of Germany), and among the small states themselves. The principal lesson the author seems to draw from his analysis is the superior flexibility exhibited by the small states, as compared with the large states, with regard to their economic and industrial policies, both domestic and international.

Finally, in chapter 4 the author proceeds to look to the historical dimension to explain democratic corporatism—except in Austria, whose development in the 1920s and 30s (a civil war and a native fascist regime) deviated from that of the other small states. For all the other small states, the roots of post-World War II corporatism must be sought in the sociopolitical and economic developments of the interwar years. Following the initial presentation of the workings of contemporary corporatism, the historical analysis becomes a constituent element of the total explanation.

A final warning: this is not a book fur casual reading. The argument is taut and concentrated; it is constructed with care and every part of it has its essential place. In short, it is what a scholarly work should be; not too many come up to this standard.

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Regime Change and the Coherence of European Governments. By Mark Irving Lichbach. (Denver, CO: University of Denver Graduate School of International Studies, 1984. Pp. xvii + 158. $24.00.)

Since 1963 the University of Denver's Graduate School of International Studies has published a stimulating and eclectic monograph series on world affairs. Although not without its flaws, Mark Lichbach's book represents a solid addition to this series.

The goal of the author is to analyze changes in the authority structures of nineteenth- and twentieth-century European states, in order to determine "the extent to which European political history was dominated by three trends—democratization, bureaucratization and coherence" (p. 5). The general thesis developed by Lichbach is that regime changes have not followed a unilinear pattern of change toward greater democratization and bureaucratization (a view he ascribes to scholars he terms "developmentalists"). Rather, he sees the underlying pattern of change as one of coherence, in which changes occur in such a way that the various authority dimensions of a polity are consistent with each other.

The method of presentation is straightforward. Following initial chapters that discuss various views of regime change and the method of analysis to be pursued, separate chapters discuss changes in the democratic and bureaucratic character of European polities. Lichbach then moves on to an analysis of the coherence of these political transformations, which is followed by a brief concluding chapter. The raw data for the analysis are taken from the POLITY dataset collected by Ted Gurr, permitting the author to estimate the character of the authority structures of 49 current and historical (e.g., Bavaria) European and "European-settled" states for each year from 1800 to 1970.

The contribution of this volume clearly lies in its careful and comprehensive assembly of these cross-sectional time series data. The theoretical framework of the book, including the general procedures involved in the operationalization of the key concepts, is almost entirely taken directly from the works of Eckstein and Gurr (e.g., Harry Eckstein and Ted Robert Gurr, *Patterns of Authority: A Structural Basis for Political Authority,* New York: Wiley, 1975). The derivative nature of the conceptual framework may make some readers question whether the book offers a fair test of competing theoretical expectations (developmental versus coherence) concerning regime change. For example, while Lichbach rather unquestioningly uses Gurr's group competition measure to track changes in participation, plausible alternatives (like the extent of the franchise) that might have supported an alternative view are ignored.

There are other problems in the analysis that may lead readers to question the overall conclusions. The principal one is the simultaneous analysis of political systems that may not be comparable with respect to the hypotheses being examined. The core of the regime transformation analysis is the placing of systems into five categories (entrenchment in one of two polar states, unidirectional shifts to either polar state, and oscillation between states) for each of the several variables examined. Yet it is difficult to see how the experiences of Sardinia (1815–1860), Ireland (1922–1970), and Austria (1800–1970) are comparable and equivalent cases of democratic and bureaucratic regime changes. This problem is compounded by the author's decision to classify systems on the basis of any changes, no matter how brief, in the entire span of time and only during this time period. Thus a state that is democratic throughout the period except for a two-year intermediate period of autocracy is classified as oscillating, while a nation that is democratic throughout the period is viewed as "entrenched." In neither case will the states' experience be viewed as consistent with a long-term developmental trend toward democratization.

These sorts of problems will make many readers question the extent to which Lichbach has made his case concerning the nature of regime change in modern European systems. However, the basic presentation of the data, particularly the tables showing aggregate patterns of authority structures at ten-year intervals throughout the 1800–1970 period, should prove of great interest to scholars of both European and general patterns of political development. Although not all readers will agree with the author's findings and conclusions, they will find this book to be thought-provoking and engaging.

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Ideology, Modernization and Politics in India. By V. R. Mehta. (New Delhi: Manohar, 1983. Pp. xii + 232. $18.50.)

This book and Mehta's earlier work, *Beyond Marxism: Towards An Alternative Perspec-*

tive, follow in the tradition of modern Indian thought, particularly the works of M. N. Roy after 1947, Vinoba Bhave, Jayaprakash Narayan, and J. B. Kripalani. These theorists sought to develop concepts inherent in the thinking of Vivekananda, Aurobindo, Tagore, and Mahatma Gandhi. Central to this tradition is the attempt to restructure society so that morally autonomous individuals may find self-fulfilment in a politically and economically decentralized society. This "integralistic-pluralistic" view of society, stemming from the Indian Renaissance, represents a synthesis of East and West.

Mehta argues that both the liberal-democratic political system, superimposed upon Indian society at Independence, and Marxism are seriously flawed, particularly in their suitability to contemporary Indian society. The decision by the framers of the Indian constitution to adopt a parliamentary system, rather than Mahatma Gandhi's vision of a decentralized polity, has resulted in "cleavages between politics, economy and society" (p. x).

In Mehta's view, the incompatibility of the liberal-democratic model is grounded in the structure of Indian society, which includes various autonomous "wholes": the individual, family, village, province, nation, caste, and class. For Aurobindo the ideal relationship between the individual and the community was "complex communal freedom," described by Mahatma Gandhi as "oceanic circles." Each individual "must try to develop and realize the soul through seeking greater intercourse with other members" (p. 24). Every whole has its functions and duties, and the social system would ensure a "cosmic symphony" of wholes and their fullest development. Individuation is possible even while "the individual . . . exists within a complex of other wholes" (p. 24). What is required today is a new "functional balance" among these wholes.

The existing system, which encourages group loyalties, has led to politicization along lines of caste, linguistic, and religious community, increasing the fragmentation of society. The legitimacy of these groups is grounded in tradition, while the universalistic and humanistic values inherent in the Indian Renaissance were largely forgotten after 1947. For Mehta, the creation of Pakistan was a "surrender to communalism" that encouraged "regionalism," "linguism," and subnationalism.

Mehta's discussion of the liberal-democratic and Marxist-Leninist models updates the critiques of Roy and Narayan. It is argued that in India, the liberal-democratic system, reversed the classical process of both economic and political development. Classical economic development demanded "a flow of capital from agriculture to industry," while in India this has been reversed (p. 44). Rural conservative elites ("middle castes") have attained dominance over the middle class, and the government has failed to mobilize agricultural revenues adequately. This has retarded the social welfare of the underclasses. Similarly, classical political development envisioned the secularization of society, whereas in India, linguistic, ethnic, and regional groups have been strengthened. Marxists also failed to understand the importance of castes and religion, and consequently could not generate a relevant analysis of Indian society.

Mehta argues that Indian reformers, including Mahatma Gandhi, "could not bridge the gap between politics and society." This would have required a "concrete strategy of institution building . . . by which a strong political centre corresponding to the communal character of the nationalist movement could emerge" (p. 128). For Mehta, the creation of a strong political center along with "a powerful political ideology" (p. 188) is not contradictory to the Gandhian model. Rather, these are necessary conditions for it. He distinguishes between "survival functions," such as defense and communications at the center, and "developmental functions," which would be decentralized to promote economic development. He urges a new debate on the Gandhian model to seek appropriate institutions for modernization.

This is the most ambitious attempt to come to grips with the enormous gap between theory and practice in Indian society since the works of Roy and Narayan. Although Indian polity was influenced by Gandhian thought, particularly in the *Panchayati Raj* system and in the integration of small-scale industries into the planning models, the political and economic structure of society has become increasingly centralized. Mehta's criticism of the Western political model in the Indian context is extremely relevant, but tends to place excessive emphasis upon structural change. Chauvinistic and parochial attachments to the "wholes" of Indian society must also be blamed for contemporary social turbulence. Both Roy and Narayan, as well as Mahatma Gandhi, placed emphasis upon changes in both structures and values. Also, while Gandhian principles should serve to guide public policy in India, major changes in the political structure to adopt Gandhian institutions cannot easily be made today, as the Janata Government learned in the late 1970s. Tensions will continue as the predominant values of India's large middle class continue to clash with parochial sentiments, and as India undergoes rapid and uneven development. Nevertheless, Mehta skillfully draws upon a broad range of literature, making this work a landmark contribution to our understanding of modern Indian society.

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Human Needs and Political Development: A Dissent to Utopian Solutions. By Han S. Park. (Cambridge: Schenkman Publishing Company, Inc., 1984. Pp. viii + 270. $19.95, cloth; $11.95, paper.)

In his *Human Needs and Political Development: A Dissent to Utopian Solutions,* Han S. Park explains political development in terms of needs he claims are universal. Rejecting the idea that various cultures have values distinctive to themselves, he identifies basic needs which are species-wide. As similar needs produce similar behavior, it follows that all people will react in a similar manner to given political circumstances. Thus Professor Park's paradigm on political change applies to all and is neither enthocentric nor partial to a particular ideology.

At the outset, Professor Park evaluates several prominent theories that compare political systems, and suggests criteria by which to define political development (chapter 1-2). He next outlines his paradigm (chapter 3) and discusses each of its four stages in detail (chapters 4-7). He concludes by mentioning the consequences of his theory for the earth's resources, and by proposing how to mitigate his paradigm's negative implications about human nature (chapter 8 and postscript).

Three assumptions underlie the paradigm. The author concedes that they are debatable and must continually be reassessed. First, one consistently strives to satisfy one's needs to the greatest possible extent. Second, the prevailing needs of individuals determine the course of political development; this supposition rejects formulations such as Rousseau's "general will" that derive an interest from society as a whole. Lastly, government and politics are legitimate insofar as they can satisfy a people's changing needs (pp. 59-60).

Four drives constitues the hierarchy of needs pertaining to the first assumption: one desires foremost to survive, next to interact and to share companionship with one's fellows, then to enjoy leisure, and finally to attain superiority over others by acquiring superfluous material goods and social status (pp. 61-63). Four stages of political development correspond to these needs: regime formation, political integration, resource expansion, and conflict management (pp. 78-85). Main attributes of each stage include means of meeting prevalent needs, predominant institutions, personality traits, governing style, type of political leader, policy priorities, and the resulting political situation (pp. 86-87). These characteristics are a plausible reflection of how a government may respond to the progression of human needs. As indicated in the work's title, the paradigm dissents from utopian conclusions. Whereas many development theories portray development as inevitable, beneficial, and culminating in an ideal state, Professor Park's model specifies the conditions leading to a regression in development and argues that the most advanced development stage is also the most flawed (p. 88).

Professor Park comments that his bleak predictions are founded on an empirical rather than a normative definition of development (p. 261), and therefore describe political development as it is instead of as it should be. One may question, however, whether his theory is accurate even in empirical terms. His paradigm would not be definitive if the needs on which it is based are not universal. Of the four needs, the urge to survive is perhaps the only desire which is indisputably inherent throughout the species. The other three needs —to belong, to enjoy leisure, and to exert control—may not be essential qualities of being human, but artificial needs obtained through adverse influences; as such, it is likely that not all people possess them. The need to control other is especially questionably on these grounds. As Professor Park observes, this desire is an attribute of Hobbesian man, and he regards it as "morally undesirable but empirically undeniable" (p. 63). Subsequent political theorists, though, have argued persuasively against this interpretation of human nature; Rousseau is notable among these.

LINDA LUM

U.S. Department of State

The Political Economy of Zimbabwe. Edited by Michael G. Schatzberg. (New York: Praeger, 1984. Pp. x + 276. $29.95.)

The essays in this volume all reflect upon the formidable choices facing the Zimbabwean state. The focus on state choice in a volume on political economy is particularly appropriate in the case of Zimbabwe because of both the expanded post-independence demands placed upon the state, and the weighty character of an already extremely interventionist colonial apparatus. In fact Lee Cokorinos, in a sweeping essay on party and state formation, asserts that the Unilateral Declaration of Independence in 1965 and subsequent economic sanctions against Rhodesia marked the "creation of one of the most directed capitalist economies in the world" (p. 29).

Perhaps the most central choice facing a state is that between two basic development strategies: technocratic and populist (sometimes termed *capitalist* and *socialist*). David Gordon's lucid essay on development strategy suggests that the regime's use of ideas from both camps has judiciously balanced requirements for growth and equity. Of course, choices are not made in a vacuum, and in Zimbabwe there are constituencies loosely grouped around the two competing strategies. Writing on factions behind the strategies, Ronald Libby asserts that none of the major spokespersons of the technocratic position were participants in the seven-year guerilla war, and most are professionally trained intellectuals now working in state ministries. Their allies are businessmen and larger farmers. They did not inherit the radical Marxist-tinged ideology that grew up in rural branches during the war, and many of them are not prominent in the highest councils of the Zimbabwean African National Union (ZANU). According to Gordon, the technocrats see growth premised upon an open capitalist economy, with exports maximized and foreign private investment encouraged. Productive commercial farmers, most of them white, will remain, while there will be some land reform with encouragement of peasant production. There will also be an enlarged state apparatus in order to manage expanded social service and agricultural sectors, as well as to renegotiate and regulate relations with multinational corporations.

The populists are rooted in ZANU. Their development strategy begins with large-scale land reform, which was the primary demand of African nationalism. Government expenditure will be targeted at the peasantry, in order to reverse prior bias toward large farmers, and there will be a high minimum wage for urban workers. A self-reliant and nationally integrated economy will be built with minimal ties to South Africa. All foreign private investments will be critically assessed, as in the technocratic strategy, the state will greatly expand in order to provide basic social services and to direct the development effort.

Both factions expect a lot from the state. While it is too soon after Independence for these essays to make an assessment, the most important test of the state will be whether it rises to the occasion or begins to decay in the face of declining economies and gathering political tensions. Eukudzo Murapa provides a careful account of racial politics behind the civil service hiring practices of colonial Rhodesia and independent Zimbabwe. Yet, while he is properly concerned about the ideology of the bureaucracy in an officially declared transition to socialism, he is uncritical of the state's enormous expansion after Independence. The obvious danger is that the state will become parasitic and exploitative, regardless of ideology.

There are factions roughly correlated with competing development strategies, but we should not assume that the correlation is immutable—that an official triumph of strategy is necessarily a political triumph of faction. The fortunes of all factions will most broadly depend upon the presence or absence of democratic rights won in the liberation struggle. Both Libby and Cokorinos hint that the mass base of ZANU, the ruling party, is losing ground to the state apparatus. Ronald Weitzer, in a convincing essay on colonial and independent state security, finds a steady expansion of executive powers. He also finds considerable continuity between colonial and independent state repression. Searching for explanations, Weitzer uses what he terms a *state-centric* analysis, which assumes that the state, though far from homogeneous, has distinctive interests of its own that provide sufficient incentives for repressive measures. Weitzer does not address the question of what enhanced state power and repression means for the fortunes of the two broad factions defining policy choice, but his account does imply that factional leaders on all sides are willing to use the state's repressive apparatus when it suits their interests. Because of the overwhelming dominance of state over society, one or the other development strategy may be adopted by the executive core without much of a popular base. The result could be "reform" or "revolution" from above, with the majority progressively distanced from the state.

The outcome of these state-society struggles will be highly conditioned by growing tensions with South Africa. The essay by Carol Thompson on Zimbabwe's role in the Southern African Development Coordination Conference details Zimbabwe's extensive economic dependence on South Africa. As liberation forces gather strength within South Africa and as international sanctions are applied to the apartheid state, Zimbabwe must again choose. Prime Minister Mugabe supports sanctions knowing full well that South Africa is likely to retaliate by applying sanctions of its own against Zimbabwe. In the face of a hostile and increasingly desperate neighbor like South Africa, the Mugabe regime has recently made gestures of peace to rival Joshua Nkomo and ZAPU. This effort to bind up severe internal political wounds is made in order to meet the growing external threat. Should a modicum of internal reconciliation emerge, it will have great significance not only for the range of policy choice mentioned above, but also for the future of democratic rights and state-society relationships in general.

Most of the essays were presented to an April 1983 conference at the Johns Hopkins University's School of Advanced International Studies. A brief introduction by editor Michael Schatzberg, an essay on housing the urban poor by Diana Patel, a study of private capital's assessment of opportunity in Zimbabwe (foreign capital sees more risk than local capital) by Pauline Baker, and a lengthy bibliography by Jonathan Evans round out this extremely useful resource and guide to understanding Zimbabwe's crucial choices in the early years of independence.

FRANK HOLMQUIST

Hampshire College

Ordinary Stalinism: Democratic Centralism and the Question of Communist Development. By Ronald Tiersky. (Winchester, MA: Allen and Unwin, Inc., 1985, Pp. xiii + 209. $19.50)

Ronald Tiersky is impressed with the deadlock of Communism: the consistent failure of attempts at reform of "ordinary Stalinism," the orthodox praxis and legitimating doctrines of Communist regimes and parties. Unlike many who have observed this deadening and debilitating stasis, however, Tiersky does not regard it as inevitable. "Such conclusions are either unjustifiably exaggerated or. . .simply wrong" (p. 7). Instead, he finds ". . .in democratic centralism—evidence of *autonomous political developmental potentialities* on the basis of communism itself" (xi).

Tiersky seeks to build this argument through a combination of analysis of general trends in Communist internal politics and case studies of postwar developments in the Italian and French Communist parties, especially in the last decade. The first half of the book traces the evolution of ordinary Stalinism. Much of the analysis is conventional, but Tiersky argues that the system of Communist internal governance was the result of choices that sacrificed the dialectic premises of Lenin's original formulation and the possibility of internal diversity to the primacy of unity and the monopoly power of party, state, or leader.

The result was the emergence of the "holy trinity" of ordinary Stalinism's legitimating doctrines—democratic centralism, dictatorship of the proletariat, and proletarian internationalism—each corresponding to an appropriate level of Communist politics—party, state, or international movement. These buttressed practices at each level that eliminated all possibility of meaningful diversity, much less debate. Today, however, the latter two of these doctrines are in an advanced state of decay and democratic centralism is itself under sharp attack, at least among the nonruling parties.

It is here that Tiersky brings his case studies into play. He seeks to show that "communist political development in West European countries can be read as an internal struggle waged essentially over the reform of democratic centralism, in its large rather than narrow meaning" (p. 95). By this he intends that the core theme of ordinary Stalinism—monolithism, the attempt to transform all politics into internal politics and thus totally to dominate flanking organizations, allies, and the society as a whole—is at the heart of the conflicts and changes that have occurred in the two major nonruling parties. The parties are, it is argued, faced with a profound dilemma: developments in the Communist states and the international movement have meant that the only Communist source of legitimacy lies in the claim to have a superior form of party organization; yet it is precisely this form that cripples the parties' ability to succeed in the societies within which they compete. The choice, therefore, is between orthodoxy and a future in which the parties can be expected to "wither and decline" (p. 96), or "reforming the prohibition on faction", thereby creating the possibility of successful adaptation (p. 172).

The French and Italian parties are well chosen to demonstrate this choice and the consequences of alternative resolutions. The French party, inconsistent and probably insincere in its willingness to reform its internal practice, has condemned itself to a loss of electoral and political influence. The Italians, by contrast, have gradually but consistently pursued reform, expanding the possibilities for internal diversity even while successfully maintaining the formal prohibition on organized factions. They too, however, may have reached the limits of the attempt to change the substance of internal politics without permitting factions. Tiersky's more intimate knowledge of the French part is apparent, but he has effectively used the extensive research on the Italians to make his case.

Nonetheless, upon completing this study one remains more convinced by the description than by the analysis. Tiersky's general assertion—that reform of Communist politics can come from within—is not demonstrated. The sole case of real reform, the Italian, is possibly anomalous. Tiersky himself speaks of "the force of circumstance" and the "quality of leadership" as possible explanations (p. 96). Furthermore, the Italian party is, among Communist parties, almost unique in the extent to which it has made its organization open to permeation by the larger society. To speak of reform from within is, therefore, misplaced. Tiersky's book provides a good analytical description of Communist orthodoxy and its sources of doctrinal crisis. He offers much less for those seeking to explain change in Communist parties, and thus fails to convince us that "communist development is inevitable and the real issue is not whether, but when" (p. 176).

PETER LANGE

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Nicaragua: The First Five Years. Edited by Thomas W. Walker. (New York: Praeger, 1985. Pp. 561. $17.95, paper.)

Thomas Walker began studying Nicaragua in 1970. Since 1979 Walker has graciously served as unappointed coordinator for the swarm of social scientists bent on studying the Sandinista revolution. In addition to assisting individual scholars, he has organized numerous forums for the dissemination of their research. *Nicaragua: The First Five Years* is such an effort. The mammoth text includes contributions by 34 scholars, organized into 25 chapters, and buttressed by an introduction written by Walker. An appendix presents a summary of the report of the Latin American

Studies Association's delegation to observe the Nicaraguan elections held in late 1984.

Chapters address a plethora of topics, ranging from the armed forces, to women, to housing policy, to relations with the nonaligned movement. Despite 25 chapters there are some glaring gaps, particularly concerning the church and the counterrevolution. More problematic: Even if the book is read from page 1 through page 561, an organic view of the Nicaraguan Revolution does not emerge. The chapters are individual slices into a whole that remains elusive. The book, contains a wealth of data on a wide variety of subjects, however, which should be useful for those attempting comparative and theoretical analyses of contemporary revolutions, as well as for those who simply want to know more about Nicaragua.

Given the number and variety of scholars presenting research in the volume, there is also an opportunity to observe in comparative fashion the way "area specialists" conduct their inquiries and the resulting success they enjoy. There are some interesting studies, such as Dennis Gilbert's fascinating portrayal of the contradictory role played by the "bourgeoisie" in the Nicaraguan Revolution. Comparing his and similar successes in the volume with their less successful counterparts suggest a number of maxims for those studying revolution in a hot country: Be sensitive to time, avoid being state-centric, use multiple data sources, have a healthy sense of skepticism, opt for specificity, avoid sweeping generalizations, and pose questions that admittedly cannot be answered but that nonetheless deserve to be asked.

FORREST D. COLBURN

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INTERNATIONAL RELATIONS

Hawks, Doves, & Owls: An Agenda for Avoiding Nuclear War. Edited by Graham T. Allison, Albert Carnesale, and Joseph S. Nye, Jr. (New York: Norton, 1985. Pp. xii + 282. $14.95.)

This valuable, and in various ways provocative, book is subtitled "An Agenda for Avoiding Nuclear War," but as the editors confess, "many important causes and dimensions of war are deliberately ignored here" (p. 17). The book looks for possible steps to reduce risks of nuclear war. It consists largely of chapters focused on the possibilities of nuclear war by accident (written by Paul Bracken), by surprise attack or preemption (Richard Betts), by escalation in Europe (Fen Osler Hampson) or in the Middle East (Francis Fukuyama), and by catalytic war (Henry Rowen).

The editors frame these analyses with an introduction focusing our attention on three levels of causation (precipitating, contributory, and deep) and a conclusion admittedly caricaturing three positions: the familiar hawks who fear weakness and doves who fear arms races, and the editors' own self-characterization, "owls," who fear "loss of control and nonrational factors in history" (p. 210). The editors are owls because of their assessment that the greatest risk of nuclear war at present lies in inadvertence. Individual chapters by "specialists" (they assert that there are no "experts," because we have no nuclear wars to serve as materials for expert study or experience) speculate comprehensively on possible, if not plausible, routes to superpower nuclear war and possible measures to lessen their likelihoods.

Arguing that "the primary objective of the United States in either building up or building down nuclear arms should be to protect and defend U.S. values and institutions by *avoiding nuclear war* with the Soviet Union" (p. ix), the editors compile the conclusions of the specialists into an agenda of 10 general principles incorporating 27 "dos" and 24 "don'ts." The principles are largely unexceptionable: maintain a credible nuclear deterrent, enhance crisis stability, invigorate nonproliferation efforts, etc. Some of the specifics are more interesting: "DON'T adopt a non-first-use policy;' "DON'T pursue a comprehensive freeze;" "DO intensify the search for alternatives to deterrence."

The book repays reading by those interested in the debate over U.S. military policy, but like many products of committee delibration, it tends toward "common sense" or a "lowest common denominator of views" rather than toward innovative approaches. The book is a product of the Kennedy School's "Avoiding Nuclear War Project" funded by the Carnegie Corporation. As such, the authors were asked by the editors, all of the Kennedy School, "to assume that some deep causes and factors—such as U.S.-Soviet rivalry, superpower interest in Europe, and Third World instability—will continue unchanged" (pp.19-20). The editors say that considering possible changes in these underlying factors will require another book, but at the end of the study they venture that "the search for less conventional, more imaginative alternatives [to nuclear deterrence] must begin. Bold, creative approaches to the subject must be stimulated, nurtured, and rewarded" (p. 246).

Such approaches are ruled out of this book by its short-term and largely technical focus. Also largely absent—as it is in most writing on strategic and foreign policy—is careful attention to underlying assumptions about how policy advice should be developed and what claims to authority it can make and justify.

The volume's authors make more than the usual reference to historical events—especially to the outbreaks of the world's wars and the conduct of the superpowers' crises. And yet Francis Fukuyama, formerly of the State Department's policy planning staff and now "senior analyst on Soviet and Middle Eastern Affairs with the RAND Corp.," can assert that "wars arise less often where superpowers stakes are asymmetrical than where they are both strong and evenly balanced" (p. 118). Leaving aside the (unaddressed) question of how one would determine the strength and balance of stakes, we are offered as a factual, indeed historical, generalization a statement for which there is utterly no historical basis, in that there has never been a single superpower war, let alone enough to allow the conclusion that "such wars arise less often where . . . than where. . . ." In a similar, although less egregious, instance, Paul Bracken asserts that "indeed since 1945 [nuclear] deterrence has worked," (p. 28) ignoring the important recognition that the absence of something cannot be attributed to the presence of something else without compelling evidence of a causal connection. That evidence—that U.S. nuclear forces actually deterred the Soviets from attacking the U.S.—is not available, of course, either here or elsewhere. Indeed, the book's all-too-short single chapter on "Soviet perspectives on the Paths to Nuclear War" by Stephen Meyer makes the point that we suffer from insufficient and conflicting sources on Soviet decision making.

One wonders whether the analysis might not have profitably ventured further, into the difficult and demanding political and diplomatic context of these dangers of nuclear war, and indeed into the domestic context so significant in the Reagan years. Further, one might hope for presentation of a more developed understanding of the connections and disjunctions between empirical theory based in limited historical experience of even more limited relevance, and policy theory, which, like policy itself, must begin with uncertainty, take and specify risks, and structure analysis and choices to provide for both more venturesome trial and protective reversal in the face of error or surprise.

The editors of this book assert that "human beings can think and, through thought, take initiative to shape their social institutions. . . . The United States and the Soviet Union must discover ways to overcome history-as-usual" (p. 8). This volume struggles, in a sense, to perpetuate our nuclear history-as-usual of "successful deterrence" by offering its collective agenda. In the end, the editors pledge to "join with others in exploring the realm of ideas beyond deterrence" (p. 246). How about a Carnegie Corporation project on Creating Post-Nuclear-Deterrence Peace?

DAVID V. EDWARDS

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Economic Statecraft. By David A. Baldwin. (Princeton: Princeton University Press, 1985. Pp. ix + 406. $55.95, cloth; $15.95, paper.)

The central argument in Baldwin's study is that critics of the use of economic statecraft have not studied their case histories thoroughly and have looked at specific uses of economic statecraft, such as sanctions, through a very narrow analytic prism. To prove his point, Baldwin reexamines a number of key cases, such as the ill-fated League of Nations sanctions against Italy, the U.S. embargo of oil and scrap iron to Japan prior to World War II, the U.S. embargo against Cuba, the U.N. sanctions against Rhodesia, the post-World War II East-West trade embargo, the U.S. economic sanctions against Iran following the seizure of the U.S. embassy in 1979, the partial grain embargo against the USSR following the invasion of Afghanistan, and the Reagan administration's use of economic sanctions against the USSR following the imposition of martial law in Poland in 1981. He also deals, in a more limited way, with Soviet uses of economic sanctions against Yugoslavia, Albania, and China.

In calling for a reevaluation of the utility of economic sanctions and other forms of economic statecraft, Baldwin contends that there are no fewer than nine factors that must be understood in evaluating the success or failure of these undertakings. The most important of these are the realization that targets and goals of economic statecraft are usually multiple; that the use of a tool of economic statecraft should be evaluated—both as to costs and desired outcomes—against alternative tools of statecraft such as military action; that success is usually a matter of degree and that neither perfect success nor perfect failure is likely; that imposing economic costs on a target nation for noncompliance is itself a measure of success; and that economic statecraft, when used, has important symbolic aspects that can affect a nation's image in the international arena. On this last point, Baldwin, citing Robert Jervis, asserts that the symbolic aspects of economic statecraft will influence the "psychological environments and policies" of foreign decisionmakers (p. 100).

Baldwin, in asnwering the critics of the utility of economic sanctions, goes beyond merely rebutting their arguments, however. While noting that economic sanctions lie somewhere between war and appeasement in terms of a continuum of "toughness"—and hence are open to attack from both "hardliners" and "soft liners"—he notes prescriptively that "techniques that enable policy makers to demonstrate firmness while reassuring others of their sense of proportion and restraint can be highly useful, especially to nuclear powers" (p. 105). In addition, in citing the desirability of using economic statecraft as a means of registering approval or disapproval, Baldwin notes that "economic techniques usually cost more than propaganda or diplomacy and thus tend to have more inherent credibility. Military techniques, of course, usually entail higher costs and even more credibility, but their costs may be too high" (p. 107) Baldwin concludes that "compared to other techniques of statecraft, economic measures are likely to exert more pressure than either diplomacy or propaganda and are less likely to evoke a violent response than military instruments." Hence, "they are not merely inferior substitutes for force but first-best policy alternatives" (p. 110).

Given Baldwin's perspective, it is not surprising that the often-criticized American use of economic sanctions against Iran and the Soviet Union during the Carter administration receives very high evaluations. In both cases, the U.S. demonstrated that there could not be "business as usual" while the target state undertook policies that angered the United States; that the U.S. was itself willing to undergo economic costs to demonstrate its firmness (Baldwin notes that in the case of the grain embargo the cost to American farmers was far less than alleged, and the cost to Moscow far greater); and that the U.S. was able to demonstrate its anger prudently, that is, without either driving Iran into the arms of the Soviet Union—something that might have happened if the U.S. employed major military force against Iran—or escalating the Afghanistan crisis with the USSR to the brink of nuclear war.

In looking to a general evaluation of Baldwin's study, it is clear that his discussion of the utility of Western uses of economic sanctions is by far the most useful section of his book, and political scientists will read it to their benefit. His discussion of Soviet uses of economic sanctions, however, is somewhat weaker. In the case of Yugoslavia, I would disagree with Baldwin's view that Stalin wanted to send signals to other communist countries about the danger of noncompliance with Soviet wishes via economic sanctions (p. 230). Soviet soldiers occupied all the other East European communist states except Albania, which was allied with the USSR against Yugoslavia, so there was little need to send signals to them. Indeed, this was a case where Stalin appears to have thought (incorrectly) that propaganda, diplomacy, and economic sanctions alone might topple Tito.

Other than this small weakness, David Baldwin's book, *Economic Statecraft* can be highly recommended, both to scholars in the field of international relations, and to statesmen grappling with the problem of exerting influence in a very dangerous world. Indeed, those who have criticized President Reagan for his January 1986 economic sanctions against Libya might consider changing their views after reading this book.

ROBERT O. FREEDMAN

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Law, Force and Diplomacy at Sea. By Ken Booth. (Winchester, MA: Allen & Unwin, Inc., 1985. Pp. xiii + 231. $25.00, cloth; $12.50, paper.)

Much has been written about the UN Convention on the Law of the Sea (UNCLOS III) in the years immediately preceding and following the signing of the agreement at Montego Bay, Jamaica, on December 10, 1982. This rather expansive body of literature has tended to focus either on the purely legal interpretations of the convention's articles or on the changing strategies of the principal actors, particularly the United States, during the negotiations on a comprehensive treaty. In *Law, Force and Diplomacy at Sea*, Ken Booth, an expert on the law of the sea and a member of the faculty at the University College, Wales, Aberystwyth, goes beyond these limited approaches to present a thorough examination of the changing nature of the law of the sea, particularly as it affects the strategic planning of the world's great naval powers.

The book spells out in detail the background and outcome of UNCLOS III from a strategic perspective, focusing on the major military considerations; the rights of passage through "choke points" (straits and archipelagos); and traditional rights in the seabed, territorial sea, and the exclusive economic zone. This discussion is characterized by attention to the problems of the major naval powers as they threaded their way through years of debate with those nations who argued for "creeping jurisdiction" over the oceans, or what Booth terms the "territorialization" of the sea—extending rights and duties concerning order, the exploitation of resources, and the exercise of sovereignty by coastal states. The story of how the U.S. changed its position from advocacy to one of opposition is also told in detail.

From an analysis of the 1982 convention, Booth draws four major conclusions. First, it satisfies the military requirement of the naval powers. Second, the text is a "masterly compromise" between the interests of the naval powers and the remaining members of the international community. Third, there are, nonetheless, ambiguities and potential sources of dispute with respect to the military domain. And fourth, the naval powers will judge success by what happens in practice.

It is to this last conclusion that Booth turns in the second half of the book. In it he establishes a future agenda for discussion of the military implications of the changing law of the sea. Despite his fundamental conclusion that naval interests have been well served by UNCLOS III, he argues rather persuasively that the compromises by naval powers in earlier conventions may not be enough in future negotiations on the law of the sea (UNCLOS IV, UNCLOS V, etc.). The classical paradigm *mare liberum*, or freedom of the sea, is now under attack, as more and more states argue in favor in *mare clausum*, or a closing of the sea. This push for a new ocean regime manifested itself in several ways at UNCLOS III, most notably in the provisions for a 12-mile territorial sea, the 200-mile exclusive economic zone, and the international authority for deep-seabed mining. In Booth's thinking these represent a clear sign that the future bodes ill for the world's naval powers, if not in 5 years perhaps in 50. The last chapter is devoted to a thoughtful analysis of how navies might address this trend in the years ahead.

The book is designed for multiple audiences: students of naval strategy, international lawyers, and those interested in international affairs. Despite such a multifaceted audience, Booth has succeeded in bringing clarity to a complex subject while simultaneously raising important issues for future discussion, and it is recommended reading for serious members of the three groups named above. This reviewer's enthusiasm would be even greater were it not for the frequent and sometimes caustic anti-United States commentary that finds its way into the book. Such value judgments have no place in this otherwise excellent display of scholarship.

JAMES E. HARF

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Revolution and International Politics. By Peter Calvert. (New York: St. Martin's Press, 1984. Pp. 222. $25.00.)

Social Science and Revolutions. By Stan Taylor. (New York: St. Martin's Press, 1984. Pp. 176. $25.00.)

These two books by British political scientists have little in common other than their lexical concern with revolutions. Calvert offers disappointingly discursive commentaries on the international system and on the forms, causes, and ramifications of revolution, with occasional observations about the political linkages between international and revolutionary politics. Taylor contributes an incisive and critical set of summaries of the scholarly theories of revolution written during the 1960s and 1970s; this is substantially the best book of this genre yet published.

Calvert's general objective is "to explore the nature of revolution in the context of international politics" (p. 1). He opts for a broad definition of revolution as "the forcible overthrow of a government or regime" (p. 2), a definition that accomodates wars of national liberation (chap. 3) and what are said to be the three main forms of revolutionary action: coups, guerrilla action, and urban insurrection (chap. 6). The traits of each form are discussed with extensive examples. Constituents of a theory of the internal causes and processes of revolution—for example, a sketch of the strategic stages in revolution: preparation, action, and consolidating—are scattered throughout the book. A thoughtful discussion of the psychological and social bases of revolutionary leadership and recruitment (chap. 5) is bracketed with an irrelevant and factually questionable chapter on the culture of violence (chap. 4). There is a stale quality to these chapters. For example there is no attempt to incorporate any of the arguments of theorists of revolution such as Barrington Moore, Samuel Huntington, Charles Tilly, or Theda Skocpol, among others; Huntington and Tilly are not even cited in the bibliography. In fact in a bibliography of 156 items, only 37 were published after 1970.

The international dimensions of Calvert's analysis include a discussion of force in the international system (chap. 2); an accounting of the problems revolution poses to traditional diplomacy, especially regarding the recognition of new regimes (chap. 7); and an analysis of diplomatic and military intervention in potentially revolutionary situations (chap. 8). At several points Calvert refers to dependency theories and to economic intervention, but virtually dismisses them. Since economic penetration does not involve specific acts by governments to influence politics in other countries, it is not "intervention" (pp. 165–66), and the United States is not "neocolonial" because it lacks effective checks on private investment abroad (p. 54). Theories and evidence that international banks and corporations influence both the foreign policies of capitalist states and the domestic policies of host countries, with profound implications for political stability and revolution, are not discussed.

In sum, the book has little to offer to the conflict analyst aside from some obscure but interesting illustrative materials, such as a nine-page summary of *Armed Insurrection*, a 1928 Comintern manual on urban insurrection. One may applaud the author's "principal conclusion" that "citizens should in future expect and demand of their governments a policy of enlightened self-restraint" with regard to revolutionary conflicts elsewhere (p. 200), but one should not expect to find a sustained or coherent argument that leads to this or any other general conclusion.

The core of Taylor's *Social Science and Revolution* is a set of summaries and critiques of four different bodies of theory on the causes of revolution. No favorites are played. Unlike Barbara Salert's *Revolutions and Revolutionaries* (New York: Elsevier, 1976), this book gives equal treatment to each of the bodies of theory and evidence without the a priori conviction that one is superior to another. Nor is it the author's purpose to lay waste to other theories as a preliminary to demonstrating the superiority of his own. Far from least, the summaries of the theoretical arguments are masterfully done. The reviewer is closely familiar with most of the dozen theories reviewed here, and did not find a single serious misstep in their presentation. Each is characterized fully, accurately, and with sensitivity to their nuances.

Following a brief but useful intellectual history of the concept of revolution, a cluster of "sociological" theories of revolution are evaluated. These divide into two sharply distinct groups: functionalist theories, rooted in Parsonian analysis and represented by Chalmers Johnson; and conflict-coercion theories, based on some of Marx's arguments and represented here by Moore and Skocpol. Particularly useful is the reduction of each theory to a schematic model showing the alternative sets of conditions said to lead to each specified kind of revolutionary or nonrevolutionary outcome. These sketches alone are worth the price of admission for anyone teaching a course on revolution or looking for a quick summary of the underlying logic of the theories. Johnson's theory of revolution is faulted because of the profound difficulties of making its concepts operational, whether in quantitative or case studies. Moore and Skocpol are criticized for their neglect of voluntarist psychological and political causes of revolution. The summary judgment of Skocpol is particularly succinct and telling:

> Her claims concerning inevitability and the primacy of structural variables could only be fully substantiated if she had developed her model independently of particular cases and then demonstrated its predictive powers . . . or if she had transformed conclusions based upon case studies into hypotheses which could be tested by reference to other causes. (p. 50)

Taylor's analysis of "socio-psychological" theories distinguishes between those which rest on Festinger's theory of cognitive dissonance, represented here by the work of James Geschwender and David Schwartz; and those using frustration-aggression theory, in particular Davies' J-curve theory of revolution and Gurr's relative deprivation theory of political violence. The early macrocomparative research inspired by these theories is carefully summarized and critiqued on the familiar ground that such empirical work could not demonstrate directly the existence of any connection between psychological processes and the occurrence of political violence. Unaccountably missing is any mention of Edward N. Muller's theoretical and microempirical research on the social psychology of aggressive political behavior, which developed out of just such an empirical assessment of these theories. Taylor also makes another point often overlooked in critiques of these theories: they are concerned not with explaining revolutions per se, but with the extent of political violence and individual participation in it.

The author's evaluation of "economic" or rational-choice theories of revolution is less satisfactory, because it is limited to three early statements of such theories—by Ireland, Tullock, and Silver—and does not take into account subsequent theoretical and empirical attempts to rescue rational explanations of revolutionary action from the free-rider problem.

Two distinct "political" theories of revolution are reviewed: Huntington's functionalist theory (actually a theory of political instability, which subsumes revolution) and Tilly's resource mobilization approach. As in chapter 2 figures are used to summarize the essential arguments. Taylor praises these theorists for their efforts to link economic, social, and political variables, and to go beyond narrow etiology to consider both the development of revolutionary situations and their outcomes. The principal criticism of Huntington is his failure to give enough weight to the ability of regimes to maintain themselves by coercion. Tilly is faulted both for tautological elements in his argument and for his exclusion of institutional and sociopsychological factors.

Since Taylor often criticizes one body of theory by pointing out that it fails to take account of variables that are central to other theories, one might expect that his concluding chapter would move toward synthesis. Instead it becomes mired in a metatheoretical discussion about the ideological basis of scholarly theories of revolution. He begins with the accurate observation that revolutionary phenomena are defined very differently in the theories reviewed. He next assumes that "Revolutions are not naturally-defined phenomena comparable to those studied in the natural sciences" and concludes that any attempt to define them is therefore inherently ideological (p. 152). The conclusion does not necessarily follow. While there is some inherently irreducible element of "interest" in a scholar's initial definition of the phenomena under study, the central purpose of a theoretical definition is to identify an unambiguous subject matter that is susceptible to analysis and explanation. There can be little doubt about the inherent observability of such explananda as "participation in revolutionary movements" or "extent of political violence" or the "forcible overthrow of governments or regimes." The differences among definitions of revolutionary phenomena reflect differences in disciplinary paradigms and the personal interests of researchers more than they do ideological dispositions. Given an operationally defined subject matter, the processes of explanations and comparative research can proceed irrespective of the interests that may have influenced the initial definition.

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Consultation and Consensus in NATO: Implementing the Canadian Article. By Edwina S. Campbell. (Lanham, MD: University Press of America, 1985. Pp. xviii + 209. $26.00, cloth; $12.75, paper.)

This book discusses the origins and development of the NATO Committee on the Challenges of Modern Society (CCMS). CCMS was born out of an American proposal in response to the ever-increasing problems of protecting the environment. The idea for CCMS was based on article 2 of the North Atlantic Treaty, better known as the Canadian Article.

The idea of greater social and cultural cooperation between members of the Atlantic Alliance is an interesting one indeed, and raises several important questions. One would expect, as the preface suggests, that a book dealing with the development of international cooperation within NATO through CCMS would address the following questions. First, what are the theoretical premises and assumptions of CCMS as a cooperative international organization within the international system? Given the presence of CCMS as a cooperative organization, how does the dominant neorealist conception of international relations fit or reject the situation of CCMS? Second, what was the relationship between CCMS and other international organizations with competing jurisdictions such as the Organization for Economic Cooperation and Development (OECD), the UN, and the Council of Europe? Third, what was the importance of external environmental factors and the inter-Alliance pressures on the internal decision-making process within NATO as well as within CCMS? Finally, what does the failure to implement the Canadian Article indicate about either the implicit value of the proposal or the workability of the idea of non-military cooperation in a strategically-defined alliance?

Consultation and Consensus in NATO unfortunately fails to answer any of these questions sufficiently, and addresses only the question of internal political processes with some degree of clarity and comprehensiveness. The book provides virtually no significant theoretical discussion of international organization. The author fails to put the existence of CCMS into the context of its international environment and in terms of its implications on international order and state cooperation on non-military issues. Campbell does best with the query on the subject of the internal decision processes of the Committee and of NATO. She describes the impact of the American influence upon the formation as well as the operation of CCMS. Finally, by initially maintaining that CCMS was a workable solution to international environmental problems. Campbell fails to acknowledge and therefore address the question of the value of CCMS.

Campbell's study makes three modest contributions to our understanding of the subject of the principles behind the Canadian Article and the relationship between the U.S. and NATO. First, she does present, although roughly organized and questionably documented, a case study of the development and operation of CCMS. *Consultation and Consensus in NATO,* while not comprehensive, sheds some light upon the process and progress of CCMS, and upon the subsequent unsuccessful implementation of the Canadian Article. Second, the author features an interesting look at an example of the phenomenon of "close-of-business" (COB) attention in the bureaucracy by citing the behavior of U.S. State Department foreign service officers. Finally, the book gives us some idea, albeit with limited predictive quality, as to how an issue with strong or sole U.S. backing in NATO will be handled.

Consultation and Consensus in NATO disappoints and frustrates the reader much more than it rewards him with understanding and knowledge of cooperation within NATO. The lack of a theoretical base throughout the book is most disappointing. Along with organizational unrulyness, the book has many irrelevant statements and quotations, some of which have incomplete citations or no citations at all. In addition, the prose quality of the book is so choppy that reading it becomes something of a chore. Additional criticisms of Campbell's work center on her less than professional and impartial scholarship. She makes little attempt to neutralize her adoration of the policies of the first Nixon administration and her disdain for liberals who call for stringent environmental controls: "Their case was similar to that of the supporters of complete nuclear disarmament, and it did the environment the same disservice which the all-or-nothing anti-nuclear movement has done the supporters of arms control" (p. 50).

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Measuring Military Power: The Soviet Air Threat to Europe. By Joshua M. Epstein. (Princeton: Princeton University Press, 1984. Pp. xxx + 288. $22.50.)

Accurate threat assessment in American defense planning has long been hampered by overconcentration on such static measures as overall force size, known or assumed weapons characteristics, and similar tangibles of enemy military capability. More often than not, this narrow approach has ignored those "softer" variables relating to human performance (such as training, tactics, sustainability, and operational prowess) that could be pivotal in determining the actual combat leverage of an adversary's force posture. Although military balance appraisals based solely on the technical dimensions of enemy weapons inventories may or may not routinely overstate the resultant "threat," they can be counted on to yield a misleading portrait of enemy capability if viewed in isolation from operational context.

Using Soviet tactical airpower as a case in point, Joshua Epstein seeks to take on this problem by illuminating those stylistic attributes of Frontal Aviation, the Soviet Air Force's tactical air arm, that would bear most heavily on NATO commanders in the event of a major nonnuclear confrontation in Europe. His main premise is hard to quarrel with:

> Virtually the entire defense debate . . . concerns itself not with wartime outputs, but with peacetime inputs—static inventories of men and machines. Negligible attention is paid to the operational inputs involved in taking those peacetime inputs (e.g., tanks, planes) and producing a wartime output—achieving any specific military goal. (p. ix)

His avowed objective is equally laudable: To consider whether the Soviet Air Force actually possesses the needed wherewithal to execute the canonical mission commonly attributed to it, namely, destroying NATO's nuclear response capability by means of a massed and carefully-orchestrated conventional attack. The central argument of his book is that the practical capacity of Frontal Aviation in this assumed primary role is, for a variety of reasons, much less than one would gather from a superficial look at its order of battle.

As far as it goes, this is a reasonable point of view. The problems crop up in the way Epstein develops his argument. To begin with, over a third of the book is devoted to complex mathematical gymnastics that address variables (based largely on second-order assumptions about maintenance efficiency and sortie-generation capacity) whose relevance to Soviet combat effectiveness—especially during the critical early days of a conflict—the author merely asserts but does not explore. He thus begins by failing to ask the right questions. He does not adequately distinguish between Soviet peacetime and wartime practices. He gives insufficient credit to the ability of even hide-bound Soviet generals to ride a steep learning curve once the pressure is on. Most of all, he expends a great deal of needless energy manipulating "data" that are too fragmentary to support his sweeping generalizations and that have a less-than-immediate bearing on Soviet combat capability in any case.

True enough, one can advance a cogent argument that Frontal Aviation, as currently equipped and trained, would have a difficult time defanging NATO's nuclear option by conventional means alone. However, Epstein overlooks those factors that would probably have the greatest effect in blocking the success of any such operation, namely, Moscow's lack of adequate hard-structure munitions, the quality of NATO's air defenses, the poor adaptability of Soviet aircrews in the face of rapidly changing combat conditions, and the abysmal weather environment of Europe that would frustrate the precision high-angle delivery of conventional bombs—which would be required to destroy NATO's runways, hardened aircraft shelters, and nuclear storage bunkers—throughout most of the year. Had he more carefully examined the public record on Soviet tactical air equipment and operating routines, he could have advanced a more plausible case for his hypothesis—and one that did not need all the mathematical contortions to stand on its own feet.

Leaving aside this misdirected number-crunching, Epstein does not provide much accounting of Soviet tactical air competence, even in qualitative terms. Indeed, for all its claim to empiricism and analytical rigor, it is remarkable how little of a factual nature his study provides on the Soviet tactical air forces. He offers a potpourri of interesting quotations from the Soviet press about various issues that Soviet pilots and commanders periodically complain about, yet he presents no systematic treatment of Frontal Aviation as a combat organization, how it fits into Soviet theater war planning, or what it can and cannot do with its diverse assets in specific mission profiles.

Although the book purports to undertake a "close and careful job" (p. xx) of examining Frontal Aviation, its conclusions rest on no more than a couple of dozen translated Soviet articles randomly scattered over a number of years, most of them concentrated around 1977. At best, this material offers only a coarse-grained snapshot of certain aspects of Soviet Air Force activity that the Soviets at that time found appropriate to depict in their open literature. It certainly does not give us an aggregate or even a representative partial picture of Frontal Aviation operations and training today. One gets the impression that the author based his analysis entirely on what was available to him in English, which borders on looking for one's keys where the light is best. At no point does he critically allow for the various internal functions of the Soviet material he draws upon. Instead, he repeatedly accepts the sources he cites at face value as straightforward reflections of underlying Soviet reality. Necessarily, the result is a skewed image of Frontal Aviation. Not only does it exclude treatment of some important (and documentable) Soviet operational weaknesses that would affect Soviet employment potential even more acutely than the maintenance inefficiencies he postulates; it also overlooks many of the persistent strengths of Soviet tactical airpower that must be considered in any assessment of offsetting deficiencies.

Had the author provided a more thorough account of Frontal Aviation's operational complexion (even within the limits of the available evidence) and shown a way of applying metrics to those intangibles that really matter—aggressiveness, leadership virtuosity, tactical acumen, adaptability under stress, and the like—he might have produced a real breakthrough toward greater sophistication in assessing Soviet combat capability. As it stands, he has succumbed to the common error of measuring with a micrometer, marking with a grease pencil, and cutting with an axe. In the end, his judgment that Frontal Aviation would have a hard time carrying out a successful air operation against NATO's key rear-area targets emerges less from his quantitative manipulations than from his largely intuitive assumptions about Soviet maintenance efficiency and aircrew skill. The judgment itself is scarcely objectionable and, indeed, is widely shared by many of those defense professionals whom the author indiscriminately castigates as "bean-counters." Yet Epstein is deluding both himself and his audience if he believes that this conclusion stems from anything more than intellectual Kentucky windage, at least to the extent his analysis has been carried in this book.

To recall an old phrase of Stanley Hoffmann's, *Measuring Military Power* can best be summed up as a well-intentioned misstep in the right direction. Clearly the author is to be commended for having highlighted the need for a kind of analysis that is long overdue in the field of threat assessment. However, this book offers far too little substantiated insight to be of much practical value to U.S. force planners—even those who share Epstein's predilections and could sorely use the help. A "close and careful" treatment of Soviet tactical air strengths and vulnerabilities is very much the kind of volume that would stand in welcome stead in the defense research community. Unfortunately, it is also a volume that remains yet to be written.

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The United States and Multilaterial Resources Management. Edited by Robert S. Jordan. (New York: Praeger Publishers, 1985. Pp. ii + 200. $25.95.)

The majority of the eight essays in this volume stem from a conference on "The United States and Multilateral Resource Management" held at Brigham Young University in 1984. Renninger begins the volume with a brief introduction in which he presents the major issues: (1) the extent to which multilateral approaches to managing marine mineral, energy, and food resources serve or advance U.S. national interests; (2) the current trend in U.S. policy in favor of unilateral and bilateral approaches as opposed to multilateralism, and the trade-offs involved in these choices.

The energy issue is examined by Joan Edelman Spero in "Oil and the Cartel Power," and by Richard J. Kessler in "No Fuel for Development: United States Policy toward Managing International Resources." Both authors provide exceptionally exciting overviews of the changing structure of the international energy market. However, Edelman Spero's essay, like several other chapters, fails to focus directly on the supposed theme—the question of how multlilateralism in resource management can serve U.S. interests.

In a richly documented, descriptive contribution, "American Multilateral Food Diplomacy," Hopkins and Puchala show how the policies of major food organizations can serve U.S. food goals. Janet Schmidt addresses "Some Issues in the Multilateral Management of Food Resources." Her findings that "the United States cannot unilaterally ensure adequate levels of price stability and food security" (p. 90) whet the reader's appetite for policy-relevant prescriptions.

The weakest point of the essays mentioned above lies in their lack of an explicitly theoretical approach, although what these authors lack in theoretical perspective, they make up for in their impressive display of knowledge of the issues.

In many ways, the most interesting essay is that of Robert A. Coate, who provides a remarkably lucid demonstration of the linkage between the International Seabed Authority (ISA) and the call for New International Economic Order (NIEO). His effort to explain ISA and NIEO with reference to Wallerstein's "world system" theory is intriguing, although not everyone will be comfortable with his attempts to tie the marine mineral issue to Wallerstein's mega-theory.

In the final chapter Jordan underscores the volume's theme: In food, energy, and marine minerals, the U.S. can not work out its problems in isolation from the states affected by these issues.

Not all chapters are equally enlightening. The volume's sketchy historical treatment and limited theoretical insights preclude its emergence as a pioneer on the subject. On balance, though, this is an interesting and worthwhile contribution to the international political economy and U.S. foreign policy literature. Several chapters would make excellent assigned reading for graduate and undergraduate courses, especially those on energy and food resources.

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People in Space: Policy Perspectives for a Star-Wars Century. Edited by James Everett Katz. (New Brunswick, NJ: Transaction Books, 1985. Pp. ix + $29.95, cloth; $14.95, paper.)

As this review is written, the search continues for remains of the space shuttle

Challenger, which lies in ruins on the ocean floor. With this tragedy the future of the American space program itself seems submerged in doubt. Even before the shuttle disaster, a national space commission was considering future goals for American space policy. Clearly now some soul-searching assessments of our future in space are inescapable. Against a backdrop of policy transition this collection of essays can lay claim to special attention. From a policy perspective, however, it is impossible to ignore some of this volume's significant shortcomings.

Why should space be of interest to political scientists and policy analysts? A number of the essays in this book suggest answers. They highlight issues of direct interest to political scientists: the international implications of the militarization of space; the future of international organizations and agreements for the regulation of extraterrestrial resources; international disputes over the use of remote sensing data or over the allocation of increasingly scarce space within the geostationary (Clarke) orbit. Further, these essays direct our attention to the politics of large-scale R&D projects; to the sociology of spaceflight as a major public policy "movement"; and even to the politics underlying the search for extraterrestrial intelligence. As James Katz notes in his introductory essay, many of the policy issues underlying spaceflight have been ignored by social scientists, and as public funding has declined for the space program, so has much analytic interest.

At the same time that this volume directs our attention to important issues for political analysis, the utility of these essays for such analysis varies widely. Some, like those of former astronaut Harrison Schmitt and John Joseph Moakley are rather empty polemics for particular space policies. (Schmitt argues that as a counter to the Soviets we should simply make a commitment "to go into space and stay there permanently." Moakley offers a denunciation of the Reagan administration's space-based defense or "Star Wars" program.) Some of the essays offer simple descriptions of current space activities without much in the way of analytic elaboration. There is an inventory of international agencies devoted to orbital telecommunications; a chronicle of international agreements covering space exploration and celestial exploitation, and a listing of disputes that have arisen regarding remote satellite sensing.

Among the most interesting analytic essays are Nathan Goldman's exploring the politics behind the Senate's rejection of the 1979 Lunar Treaty, George Robinson's questioning the power of legal agreements to regulate future space settlements, and the opposing essays by William Sims Bainbridge and James Katz on whether popular movements or bureaucratic politics are the driving force behind programs of space exploration. Despite the subtitle of the book ("Policy Perspectives for a Star-Wars Century") readers interested in an analysis of space-based defense issues will find little to learn from here, and would be well-advised to look elsewhere for a more useful treatment.

Ironically, one of the issues ignored in this book is defined by its title. Nowhere in these essays is there an explicit argument for people in space, nowhere a defense against purely automated approaches to exploration and science-based space research. This question is precisely at issue in the future of the space program. It has been dramtically restated in the space shuttle disaster.

Books like this are suffused with a special kind of technological optimism. It no doubt reflects the psychology of most of the authors, and it is a major drive behind the compilation of a reader such as this. Yet it is important to realize that from a policy standpoint, technological optimism is not programmaticaly neutral. It tends to bias its adherents to discrete policy positions regarding the uses of space and the pace and scale of those programs we pursue in its exploration. For example, Hans Mark bases his support for space-based defenses almost solely upon Arthur Clarke's maxim of technical optimism: "When a distinguished scientists says that something is possible, he is almost certainly right. When he states something is impossible, he is very probably wrong" (p. viii). Similar psychologies closely attend endorsement of manned-centered approaches to exploration—including the construction of large space stations and human colonies in space. To be sure, there are some essays critical of the Star-Wars program, and some which do not regard new space technologies as unalloyed blessings, but the major problem ignored throughout this book (and one that has plagued the space program from its beginnings is precisely that of balancing and containing its reinforcing psychology. How can political support for space exploration be sustained without the psychology of exploration overinflating the policy and distorting its thrust?

The American space program has been unable to locate a stable policy "niche" between the "driving dream" of manned missions at an accelerating pace and what would probably be a marginal (and politically vulnerable) policy of purely science-based instrumented flight. This is a peculiar dilemma of space policy. It is not, as this book declares in its preface, "time for policy analysts to move from viewing space as an exceptional phenomenon to simply a part of everyday life" (p. 1). Indeed, it is just such an assumption that makes books like People in Space implicit promotions of a policy doctrine more than detached exercises in policy analysis.

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Korea and the United States: A Century of Cooperation. Edited by Youngnok Koo and Dae-Sook Suh. (Honolulu: University of Hawaii Press, 1984. Pp. xiv + 397. $37.00.)

As a global superpower the United States has forged innumerable sets of bilateral links with nations large and small. One of the more noteworthy of these is with Korea. Not only did the United States fight a war in Korea, it has also committed itself to the defense of South Korea, a commitment that is embodied in a mutual defense treaty as well as in the stationing of 40,000 U.S. troops in that country. Moreover, South Korea has emerged as America's seventh largest trading partner.

In 1982, in commemoration of the centennial of diplomatic relations between Korea and the United States, a scholarly conference was held in Honolulu under the joint auspices of the Center for Korean Studies of the University of Hawaii and the American Studies Institute of Seoul National University. The volume under review is an offshoot of that conference. Sixteen scholars from South Korea and the United States examine the whole gamut of American-Korean relations. The principal topics discussed are the genesis of American interest in Korea, mutual perceptions and images, the American–South Korean alliance system, the role of the other Pacific powers, economic relations, and the impact of American-Korean relations.

Ten of the authors are political scientists; the remainder represent sociology, economics, business, law, history, and philosophy. Such a diversity in the authors' training and interests helps account for variations in the approaches used and in methodological rigor. Nonetheless, the overall caliber of the papers remains high; taken together, they present an excellent overview of the track record of Korean-American relations from their inception to 1982.

The relations officially commenced with the signing of a treaty of amity, commerce, and navigation in May 1882 at the Korean port of Inch'on by Commodore Robert W. Shufeldt of the U.S. Navy and Sin Hon, president of the Royal Cabinet of the Kingdom of Korea. Earlier attempts by the United States forcibly to open Korea had failed in the face of stiff resistance by the Koreans, and it was through the mediation of Chinese Viceroy Li Hung-chang that Shufeldt finally succeeded in negotiating the treaty. The role of China in the early stage of American contacts with Korea not only bespoke China's influence in Korea, which was soon to be eclipsed by Japan's, but also foreshadowed the Sino-American military confrontation there seven decades later.

Despite their initial resistance, Korea's ruling elite, particularly the king, enthusiastically welcomed Americans to their soil, perceiving them as a counterweight to the predatory maneuvers of Korea's powerful neighbors. Cultural differences and wishful thinking had combined to spawn the hope that America would help Korea preserve its independence, for Article 1 of the 1882 treaty provided that "if other Powers deal unjustly or oppressively with either Government, the other will exert their good offices . . . to bring about an amicable arrangement, thus showing their friendly feelings." The Koreans were rudely disillusioned in 1905, when the U.S. in the secret Katsura-Taft memorandum, recognized Japanese suzerainty over Korea in return for Japanese recognition of American supremacy in the Philippines.

When the Americans reentered the Korean scene 40 years later, they came as liberators bent on helping the Koreans to rebuild a viable political order. However, the "temporary" division of the peninsula into two occupation zones hardened into a permanent partition. If, as many Koreans believe, America erred in that decision, it has paid dearly for the error. Not only did it fight a costly war against both North Korea and the People's Republic of China from 1950 to 1953, but it has been entangled in the Korean quagmire ever since.

With an impressive growth in South Korea's economic capability over the past two decades, however, the alliance system between the U.S. and South Korea has undergone profound change. By 1978 South Korea had attained self-sufficiency in national defense. Both direct and indirect American military aid ceased altogether. Since North Korea's arms buildup has continued unabated, however, both Seoul and Washington believe that the American military presence in South Korea, coupled with the mutual defense treaty between the two countries, serves a useful deterrent function.

In the realm of economic relations, too, the nature of American-South Korean ties has been transformed from "unilateral asymmetry" to "bilateral symmetry." No longer a recipient of signfiicant American aid, South Korea has emerged as one of the principal trading partners of the U.S. Not only has South Korea stopped being a burden on the U.S., but Seoul has become a formidable competitor of Washington in the American market and beyond.

While the Koo-Suh volume elucidates these and other aspects of Korean-American relations in an authoritative fashion, it is not totally devoid of problems. First, its treatment of North Korea leaves something to be desired, not because its chapter on North Korea is flawed in any way, but because it devotes only one chapter to that significant half of Korea. Second, anyone who is looking for an in-depth analysis of friction between Seoul and Washington is bound to be disappointed. Neither Syngman Rhee's friction with Washington in the waning days of the Korean War nor Park Chung Hee's difficulties with Washington over human rights and lobbying efforts receive any sustained attention. Finally, one can detect the understandable tendency of South Korean-based scholars to exercise self-censorship.

All in all, this is a welcome addition to the growing literature of Korean studies. An impressive testimony to the pervasive impact of the U.S. on Korea is that all of its 16 authors, of whom 15 are of Korean descent, have received their graduate training in the U.S.

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The Soviet Union and Strategic Arms. Robbin F. Laird and Dale R. Herspring. (Boulder, CO: Westview Press, 1984. Pp. x + 160. $28.50, cloth; $16.95, paper.)

Most studies of the Soviet Union and strategic arms fall victim to at least one of three major methodological flaws. First, some studies fail to approach strategic issues from within the contexts of Soviet perspectives and Soviet strategic culture. These studies inevitably see Soviet strategic policy as "aggressive" and dominated by a desire to achieve nuclear superiority. Second, some studies fail to acknowledge even the possibility that the Kremlin's strategic doctrine and policy may have changed over time. These studies blithely maintain that formulations of strategic doctrine and policy that were valid in the 1960s remain valid today. Often they use quotes mined from journals of that earlier period to "prove" points about Soviet strategic doctrine and policy in the 1980s. Finally, some studies eloquently support one or another thesis about Soviet strategic doctrine and policy, using Soviet quotes and weapons deployments as proofs while ignoring or rejecting the legitimacy of quotes and deployments that tend to support alternate theses. In short, this third methodological shortcoming essentially allows some studies to overlook the existence of disagreements within Soviet decision-making elites over strategic doctrine and policy.

Happily, *The Soviet Union and Strategic Arms* by Robbin F. Laird and Dale R. Herspring avoids these methodological pitfalls, and in so doing presents a lucid, balanced, and objective assessment of the Kremlin's attitudes towards nuclear weapons in the age of strategic parity. Laird and Herspring examine a large quantity of qualitative and quantitative evidence in this rather short book. Equally important, they examine the entire spectrum of evidence, not just those segments that would tend to make the USSR appear an "evil empire" or a "threatened innocent."

Indeed, the real strength of the Laird-Herspring effort is the willingness of the authors to come to grips with the range of information available on Soviet strategic policies. To their credit, they attempt to develop a set of explanations about Soviet policies consistent with that range of information, and to a great extent, they succeed. They underline the uncertainty that pervades many of the USSR's calculations about nuclear peace and nuclear war, and they provide thoughtful and worthwhile discussions of Soviet perceptions of the political utility—and limits—of nuclear weapons. Additionally, Laird and Herspring offer an insightful but overly brief overview of Soviet perceptions of U.S. strategic policies, an area of inquiry too often overlooked, but nevertheless critical if one wishes to comprehend the intricacies of the Kremlin's own strategic doctrine and policy.

On the whole, *The Soviet Union and Strategic Arms* succeeds well in its stated purpose of providing "a general overview of the Soviet strategic arms effort" (p. 1). Ironically, however, if there is one area in which the book may be criticized, it is that the authors were sometimes too intent on providing an overview. On some occasions, issues of major debate should have been addressed in greater depth than they were. For example, the Soviet approach to the conduct of nuclear war probably deserves more attention than the 18 pages devoted to it. This is not to find fault with the authors' observations and conclusions about the Soviet approach to the conduct of nuclear war, but to argue that, in places, more depth should have been provided to the overview.

This criticism, however, is quibbling. Laird and Herspring have written an objective book, devoid of polemics, on a most critical issue. They offer explanations and interpretations of Soviet behavior in the strategic nuclear arena that are consistent with the range of evidence available in that field. In short, they have provided a real contribution to the understanding of the Soviet strategic arms effort.

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Soviet Military Interventions Since 1945. By Alex P. Schmid, with case studies by Ellen Berends and foreword by Milton Leitenberg. (New Brunswick, NJ: Transaction Books, 1985. Pp. xix + 223. $29.95.)

Military Intervention in the Third World. Edited by John H. Maurer and Richard H. Porth. (New York: Praeger, 1984. Pp. xix + 239. $29.95.)

Anyone picking up *Soviet Military Intervention Since 1945* would be well advised to turn immediately to page 123, for it is only here that the authors attempt to set out for the reader their working definition of *intervention*. They define by listing 11 categories of activity ranging from the mobilization of troops in border areas to the direct engagement of Soviet combat troops. The book is divided into four parts: The first part deals with intrabloc interventions (i.e., Soviet interventions in socialist states), the second with interbloc interventions (i.e., against core Western states), the third with extrabloc interventions (i.e., in the Third World); the final part consists of conclusions and projections. Each of the first three parts begins with a general survey by Alex P. Schmid, followed by case studies by Ellen Berends (both researchers at the Centre for the Study of Social Conflicts, State University of Leiden, the Netherlands).

The introductory summary in chapter 1 traces the incorporation of Eastern Europe into the Soviet orbit, but no real attempt is made to explain Soviet motives and behavior, and the larger global context is ignored. The United States, France, and Great Britain are barely mentioned. There are five cases in this section: the incorporation of the Baltic states into the USSR, the East German uprising of 1953, the 1956 Hungarian uprising, the Prague Spring of 1968, and the Sino-Soviet border dispute during the 1960s. Most of these case studies, like those in the other sections, are little more than short descriptive essays with no overall framework of analysis tying them together. There are four interbloc case studies: the Iranian crisis (1945–1946), the Greek Civil War (1944–1949)—which is presented as a case of "nonintervention"—the Korean War (1950–1953), and the Soviet role in the allied occupation of Austria (1945–1955). The one extrabloc case study is the Soviet intervention in Afghanistan.

For case studies of Soviet military intervention to have any value, they need to be comparative in focus and structured, employing a single, consistent framework of analysis. Unfortunately, this book falls down on each of these counts. It is also a pity that each of the case studies concerns Soviet interventions in states that form what one might term the USSR's geostrategic security belt. Thus, any generalizations one could draw from the study might not be applicable to Soviet interventions, say, in Africa or Latin America (although these are mentioned in the general surveys). The sources used are entirely secondary and not terribly comprehensive. It is certainly not the case, as is claimed, that there is no historical material "which would reveal what Stalin's intentions for the postwar period were with regard to the conquered territories" (p. 2). In places the English is clumsy, but not so as to make the basic arguments unclear.

Military Intervention in the Third World consists of a collection of essays originally presented at a conference held in December 1981 by the Foreign Policy Research Institute, and designed to offer policy options for the U.S. government in meeting the Soviet challenge to American interests in the Third World. Following an introduction by John H. Maurer comparing U.S.-Soviet rivalry in the Third World with the European powers' competition in the lesser-developed regions during the period 1875–1914, the book is divided into three parts.

The first part deals with the nature of the (Soviet) threat. Alvin Z. Rubinstein examines post-Stalin Third World military interventions, differentiating between *intervention*, defined as the "direct and intrusive projection of military power on behalf of a Third World client in order to bring about a preferred political outcome" and *involvement*, such as the provision of arms and personnel (p. 20). Examining interventions in the Congo (1960), Cuba (1960-62), Egypt (1970), the Middle East (1973), Angola (1975-76), the Horn of Africa (1977-78), and Afghanistan (1979) leads Rubinstein to a number of generalizations regarding Soviet military interventions: (1) strategic factors are most important (i.e., not ideology); (2) the USSR will intervene only to support "progressive," anti-imperialist regimes; (3) once committed, they will stay the course; (4) they show little sensitivity to regional dynamics, and (5) no consideration of the central strategic balance, but (6) will intervene even where this risks superpower relations (nos. 3 and 6 might well be questioned by the Congolese, Egyptians, Syrians, Cubans, and Angolans). Rubinstein predicts that it is likely that Syria, Libya, South Yemen, and Iran "will require some kind of direct Soviet military intervention" (p. 31). Gordon H. McCormick examines the participation of Soviet "proxies" in regional conflicts. His basic assumption, which is not adequately documented, is that Cuba, South Yemen, and East Germany operate abroad "less as independent forces than as surrogates in support of Soviet interests" (p. 45). Dov S. Zakheim looks at locally generated contingencies (e.g., in Lebanon and Central America) to which the United States should develop a positive response and forward strategy.

The second part of the book assesses constraints on the U.S. in conducting such a forward strategy. The three chapters here, by W. Scott Thompson and Andrew B. Walworth, Terry L. Deibel, and Norman Friedman, set out internal constraints (Congress, the media, and public opinion), external constraints (opposition from major security allies and friends in the Third World), and technical and logistical constraints (regarding military capabilities for intervention) that together hamper American potential to project its power in regional conflicts in competition with the Soviet Union.

The last three chapters offer policy advice to American decision makers in formulating strategies to counter direct Soviet intervention or intervention by proxies, and to respond to locally generated contingencies. Among other things Kevin N. Lewis calls for a firmer balance of resolve, William J. Taylor for the recruitment of U.S. proxies, and Michael E. Vlahos for a new set of interventionist strategies whilst educating the American public to accept the concept of American global leadership. One finally puts this book down with a sense that it is designed to offer policy prescriptions for the United States to regain the global dominance it enjoyed from the mid-1940s through the late 1960s. It would have been useful to have a concluding essay tying all the arguments together—and it would also have been useful to have a chapter assessing the potential of diplomatic solutions to regional conflicts (i.e., the *Contadora* process, a peace conference for the Middle East, and U.N. sponsored negotiations over Afghanistan).

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Arms and Oil: U.S. Military Strategy and the Persian Gulf. By Thomas L. McNaugher. (Washington, D.C.: The Brookings Institution, 1985. Pp. xiii + 226. $26.95, cloth; $9.95, paper.)

The Persian Gulf rests at the core of the Middle East, the part of the Third World that is increasingly the new international heartland. Possessing over 60% of the world's proven reserves of oil, this region, sometimes referred to as the Petroleum Gulf, also carries enormous geostrategic significance. It is in the gulf where both superpowers stand in close confrontation, and where political turbulence is the order of the day. While the Soviet Union maintains an uneasy military occupation of Afghanistan only 300 miles from the gulf, the Iranians and Iraqis continue to fight for hegemony of the gulf itself in one of the most punishing and painful wars of the century. Meanwhile, traditional political systems such as those that mark Saudi Arabia, Oman, and the five gulf shaykhdoms struggle to survive against increasingly difficult odds. The United States has clearly indicated that it considers the gulf to be within its sphere of "vital interests," and has served notice that it will not hesitate to involve itself militarily if these interests are threatened by "any outside force."

Given this critically important situation, a flurry of books has recently appeared, most of them generated out of think tanks in Washington. These analyze in great detail the military dimensions of American policy toward the gulf states. The emphasis continues to be on military tactics, military strategies, and military scenarios of all kinds involving American muscle and movement. Consultants have written thousands of pages indicating how the United States can move its forces to the region with the greatest dispatch in the fastest possible time. Thus, most discussions of the politics of the gulf have focused on Rapid Deployment Joint Task Forces (RDJTFs), Central Commands (CENTCOMs), and Readiness Commands (REDCOMs). Also, much American discourse on the gulf concerns itself with terms such as *prepositioning, tripwires, preemptive strikes, intratheatre lifts, air interdiction campaigns, hit-and-run tactics,* and *surging the force.* Important regional states such as Saudi Arabia are viewed in these same terms, witness the emphasis upon SANGs (the Saudi Arabia National Guard) and SNEPs (the Saudi Naval Expansion Program).

The book under review is no exception. A research associate at the Brookings Institution, Thomas McNaugher focuses his study upon the military dimensions of gulf politics. However, McNaugher successfully suppresses the urge to count military beans, and instead attempts to recognize the social and political environment in which the military must operate. His effort to do so is both a success and a failure. It is a success in its very recognition of the importance of understanding the sociopolitical environment within which any military strategy must be framed. Moreover, it provides some sober, informed, and sensitive conclusions—conclusions that need to be carefully considered by foreign-policy makers in Washington.

The author stresses the continuing importance of the gulf to American national interests. He recognizes the complexities of the social and political systems that compose the littoral states; discusses the subtle and intertwining network of relations that marks internal, interregional, and international politics—a network that can easily unravel; and understand that the precarious stability that marks the gulf must be fundamentally understood "from the inside out" (p. 91). It is this last contribution touching on policy recommendations that is the most significant.

McNaugher convincingly states that "the United States can charge hastily into this region only at considerable expense, perhaps even at its own peril" (p. 2). He concludes that American policy must be based on a cautious, low-key approach designed to work quietly in support of regional cooperative security efforts. In particular, this means "understated" support and encouragement for the Gulf Cooperation Council (GCC). Any heavy military or political presence will only hasten the collapse of the moderate, conservative systems now in place. In the end, American policy must be delicately made, flexibly arranged, and politically sensitive. Military muscle may rest in the background, but deeply ingrained social and political problems cannot be bombed into submission or blown away. This, for example, is the lesson that America could learn from Iran and Lebanon, that the Soviets might learn from Afghanistan, and that Israel still needs to learn in South Lebanon.

Despite these important and informed policy conclusions, this book falters when it comes to examining the dynamics of the internal social and political systems that compose the eight gulf states. The reader must then accept at face value the writer's general conclusions concerning the primary issue of political stability. There is no discussion of current leadership problems and the thorny issue of succession, and very little attention paid to such gathering forces as the Islamic revival and the increasingly disaffected middle classes in the region. If the gulf region is to be understood from the inside out, then, of course, it is essential for the writer to provide more inside analysis.

Finally, the author fails to draw any conclusions with respect to American policy concerning the emotional Arab-Israeli issue, an issue that bleeds over into the gulf. There is a very large Palestinian population residing in the gulf countries; Kuwait alone is the home of nearly half a million Palestinians. The close American association with Israeli policy is a major political problem in the gulf, yet there is no discussion of what American policy should be with respect to this fundamental issue. Also, after explicitly recognizing the fundamental long-term strategic importance of Iran, the author seems to support without question the American tilt to the Iraqi position with respect to the Iran-Iraq war. The population of Iran is twice that of all the other Gulf states put together, and its military forces and revolutionary ideology are perhaps the most powerful in the region. In its visible tilt to the Iraqi side (ironically joining the Soviet Union in so doing), is not the United States once again betting on a long-run losing regime? The author fails to discuss this issue seriously.

In the end, however, this book is a major contribution. Although he does not analyze them in any depth, McNaugher does understand the central significance of domestic social and political forces in the region. U.S. military strategy can only be understood within this complex context. The author seems to agree then with the admonition of Christopher Van Hollen that the United States must not attempt to "engulf the Gulf." If this were to be attempted, it could not only suck American into a Middle Eastern military quagmire located in the backyard of the Soviet Union, but also could unleash forces of political violence that would quickly sweep away the political systems now in place in the traditional gulf states. This study soberly assesses military realities within the context of social and political systems under great stress. American foreign-policy makers would do well to read it with care.

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The Geography of Peace and War. Edited by David Pepper and Alan Jenkins. (New York: Basil Blackwell, 1985. Pp. 222. $45.00, cloth; $14.95, paper.)

Political Geography: Recent Advances and Future Directions. Edited by Peter Taylor and John House. (Totowa, NJ: Barnes and Noble Books, 1984. Pp. vi + 239. $27.50.)

Although quite different in substance and intent, these two collections offer the reader a representative sampling of current research directions in political geography and politically relevant geography, respectively. The Taylor-House volume grew out of an international conference of political geographers held at Oxford in July 1983. It was prepared on a very tight schedule, so as to be available at the International Geographical Union (IGU) conference in Paris in August 1984—to its detriment. It contains numerous errors in typography and several in pagination, and in general suffers from a lack of editing. In contrast, the Pepper and Jenkins volume consists of solicited papers, all but one by geographers with special expertise in some aspect relevant to peace studies. It evinces all the trappings of simply splendid organization and editing.

Taylor in his introductory essay in *Political Geography: Recent Advances and Future Di-*

rections attempts to theorize about the issue of scale in political geography by asking why politics occurs at three different scales: international, national, and local (urban). His answer, drawn from the scope-based conflict model of Schattschneider and Wallerstein's world-systems theory, is as insightful as it is controversial. Taylor provides a strong theoretical rationale as to why relations between scales and the different political meanings of geographic scales form central themes in modern political geography.

The remaining chapters reflect the renewed interest and rapid growth in political geography in the 1980s. Few, however, come to grips very directly with theoretical issues in the politics of geographic scale. Instead, most contributors seek to convince the reader and the IGU of why particular research questions should command a central place on the future agenda of the field. Readers expecting delivery on the promise of Taylor's introductory essay will be disappointed. Falling short of its intended objective, this work suffers from two maladies often found among collections. It lacks any clear sense of direction and contains essays of uneven quality. Although disappointing, it is not fatally flawed, because several of the chapters are very good indeed.

Jurgen Ossenbrugge's essay reviews and critiques public choice and social structural theories of the local state from the perspective they bring to bear on the analysis of local conflict. He argues convincingly for a theoretical approach to local conflict and urban political geography that is concerned with space both as a scarce political-economic resource and as a social construct dialectically imbedded in the reproduction of social relations.

Written from a Third World perspective, Bertha Becker's essay provides a valuable and provocative political geographic interpretation of the crisis of the nation-state as a manifestation of both regional legitimation crises and the current world economic crisis. Her chapter and those of Taylor and Johnston come closest to following the book's central theme.

Taylor's and Ron Johnston's chapters attempt to relate electoral geography to theories of state set within a dynamic capitalist world economy. Taylor contends that in liberal democracies there are two geographies underlying elections; one produced by what he calls the *politics of power* and the other by the *politics of support*. Basically, he calls for investigations in the changing relationships between the geographies of accumulation and legitimation. The degree to which these two geographies are congruent or contradictory can provide an alternative means of analyzing paradoxes in electoral geography. Unfortunately, Taylor fails to provide much in the way of historical analysis to support his reinterpretation. Also, his failure to recognize that the liberal democratic ideal of congruency between the two geographies is inherently unstable under capitalism undermines the analytical value of the framework. Johnston's essay is an interesting companion piece to Taylor's, in that he too analyzes the politics of accumulation/legitimation. His emphasis is on explaining the sporadic nature of elections in the Third World and the advocacy of PR electoral systems in Britain on the grounds of "fairness." Unlike Taylor, Johnston argues that in parts of the First World, and especially in Britain, electoral politics as currently practiced is not much better-suited to the promotion of accumulation than it is in the Third World. Together, these two essays clearly delineate one of the most important controversies in modern political geography: how to develop a theory of the state in which accumulation and legitimation are central, interacting processes rather than merely functional necessities. This debate has obvious relevance well beyond political geography.

Other chapters in the Taylor and House volume deserving mention are David Knight's critical analysis of the concept of self-determination and John O'Loughlin's ambitious attempt at integrating aspects of political geographic research with those in the field of quantitative international relations. Both of these essays contain excellent reviews, and should be of interest to specialists in nation building and international relations in political science. The remaining chapters are often interesting, but some are flawed by methodological weaknesses, some are unfocused literature reviews, and others appear to be pure flights of fancy or ungrounded exhortations aimed at fellow geographers.

The Geography of Peace and War consists of three parts. The first reviews processes and events that have led to the present situation of international tension, nuclear escalation, and cold war; the second examines in detail the spatial and human consequences of global nuclear war; the third attempts to identify ways in which further geographical research and teaching can contribute to the advancement of peace.

Part 1 contains the strongest contributions, with Herman van der Musten's chapter on the classification of postwar armed conflicts and O'Sullivan's on the geopolitics of the postwar world being particularly excellent. Together, these two chapters set both the tone and context for the remaining essays in this section: Tony Ives's analysis of the geography of arms dispersal, Alan Burnett's well-illustrated discussion of propaganda maps used by groups favoring and opposing nuclear deterrence, and Charles Anderson and Walter Isard's analysis of arms manufacture and regional economic dependence in the U.S.

Geographical analysis of a different kind characterizes part 2. First is Openshaw and Steadman's detailed spatial analysis of the expected death and casualty patterns that would result under possible nuclear attack scenarios for the United Kingdom. They show quite conclusively that the local geography is such that nuclear war of virtually any sort would be national suicide. Derek Elsom's chapter presents an up-to-date review and critique of the "nuclear winter" hypothesis, while Donald Zeigler questions the scientific basis of civil defense planning in the U.S. Zeigler suggests that nuclear relocation planning, the dominant civil defense strategy in both the U.S. and USSR, may actually fuel the arms race, because its success is predicated on the "home" country initiating the preemptive strike.

The final part of the volume is somewhat weaker and less coherent than the earlier sections. Most interesting is Frank Barnby's essay, which maintains that nuclear weapon-free zones and antisubmarine warfare sanctuaries are among the more implementable and appropriate "geographical" devises for averting nuclear war. Also useful is the chapter by Jenkins on geographical pedagogy and the peace movement.

Although *The Geography of Peace and War* is directed primarily to geographers, it should also find an appreciative audience in political science. It would be a very useful text in global studies and political science courses dealing with the arms race and nuclear disarmament. To my surprise, I found this a compelling book; it deals creatively and intelligently with what is clearly one of the most pressing issues in the world today.

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Collective Clientelism: The Lomé Conventions and North-South Relations: By John Ravenhill. (New York: Columbia University Press, 1985. Pp. xxi + 389. $35.00.)

Two competing myths cover the institutionalized relations between the European Communities (EEC and others) and the states of black Africa plus islands and enclaves in the Pacific and the Carribbean (ACP). One, heartily voiced by the EC Commission, is that the three Lomé conventions of 1975, 1980, and 1985 are a model for North-South relations in their skillful embodiment of both impartiality and redistributive economic justice. The other myth, expressed with theoretical coherence in the *dependencia* literature, is that Lomé institutionalizes both neocolonial exploitation and postcolonial neglect. A sound analytical study will not still these contending schools, which obviously have more at stake in the argument than their mere positions. It should, however, be welcomed by those observers and practitioners of North-South relations who are interested in a sound judgment on the difficult state of unequal relations and on ways to improve them.

Ravenhill's book is that study, and it will long stand as the authoritative work on the first two Lomé conventions and the basis for evaluating and improving performance under the third. The book has two major strengths: its clear conceptual framework, which provides both a sound basis for judging the Lomé relationships and a set of guidelines for prescribing corrections, and its wealth of knowledge and analysis about the workings of the relationship itself. Either would be a contribution: the combination makes the work exceptional.

Let us consider the second strength first. After an exposition of its conceptual framework, the book presents a history of the institutional evolution from the 1957 treaty of Rome, whose fourth part covered the European members' colonies, through the two Yaoundé conventions of association in the 1960s, to the negotiation of the first Lomé conventions were the provisions of export stabilization funds (STABEX) and of supports for EEC mineral imports (SYSMIN). Although the programs look like the commodity supports that exponents of the New International Economic Order (NIEO) have been demanding, Ravenhill thoroughly documents the limitations, use and misuse, and advantages of the two funds. The next chapter then presents a thorough analysis of the limitations imposed by the European Common Agricultural Policy (CAP)—rules of origin, sensitive products and safeguard clauses (threatened but never invoked)—on the promise of preferential access to the European market.

In the following chapter, there is an impressively comprehensive statistical study of the effect of the conventions of ACP-EEC trade, with breakdowns by ACP subgroup and comparisons with other relevant categories of developing countries, showing that the conventions did not improve the ACP trade standing, and if (as is not provable) they prevented it from further deterioration, that double negative compared badly with official promises and expectations. Finally, on trade, a chapter examines the performance of the convention countries in regard to three difficult products covered by separate protocols: sugar, bananas, and rum, where in the first two cases ACP interests were furthered by the arrangements. Another element in the promises of the treaty concerns the possibility of cooperation for industrial development. Ravenhill shows that expectations have not been met because of secondary preoccupations on the part of both the EC—fears of competition from new APC industries—and the APC—fears of infringement on sovereignty from EC conditionality. The penultimate chapter provides a many-factored analysis of the declining aid provisions of the conventions in relative terms, but also showing that Lome aid was a supplement, not a substitute, for other aid programs. Ravenhill's careful scholarship confirms many conflicting judgments about the Lomé relationship, and shows that it cannot be summarized in any simple grade. That requires a more complex concept, which is the second strength of Ravenhill's work.

For the author, the notion of a relationship held by its partners was that of collective clientalism, an exchange of complementary benefits between unequal groups of members. Weaker states would receive favored, protected status, preferential treatment, and additional aid; stronger states would receive assured sources of supply, political support, and favored if not formally preferential welcome. The inequality of the partners, as in the analysis of domestic clientelism, need not necessarily mean exploitative relations, since the parties exchange different items valued differently. Since the latter notion is also basic to bargaining literature, the relationship should fit well into institutionalized relations renegotiated at regular intervals.

Yet even in this light the relationship is not entirely successful, as the analytical chapters show. Although the parties were all probably better off with the conventions than without—the only real cost being the aid monies from the EC, the APC—there were no sacrifices on either side to produce new benefits for the two sides; nor was there any creation of new goods for the same purpose. As the Lome conventions evolved, the two sides pulled away from their initial concept rather than grow into it. The Europeans increasingly have hidden behind their rising protectionism when the convention promises access, yet they seek to protect their sources of supply of traditional products when the convention promises development. The ACP countries insist on the contractual equality of relations, yet refuse reciprocity or tradeoffs of interest to Europe. They seek above all to defend their sovereignty, not realizing that it covers economic inequality, not political equality. As a result, the relationship becomes one of aid, hence basically one of inequality, denying the promise and expectation of contractual equality.

The result, the book shows, is a dilemma. Errors abound on both sides when they—in quinquennial negotiations or daily operations—bargain out their relations. Oversold as a relation of equality, the convention falls victim to both dashed expectations and sovereign susceptibilities. Unfortunately, the clear analysis of the basic contradictions in the relationship is so thorough that it overwhelms any notion it might be corrected. Perhaps the best answer is found in such basics as more honesty in advertising—more openness and greater concern for the other party's interest in relations. The clarity of Ravenhill's analysis and the evenhandedness of his judgments help us see as deeply as possible into the complex EC-ACP relationship.

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The Militarization of Space: U.S. Policy, 1945–84. By Paul B. Stares. (Ithaca, NY: Cornell University Press, 1985. Pp. 352. $25.00.)

President Reagan's March 1983 address on the Strategic Defense Initiative put the question of space militarization directly before the public, but as Paul Stares amply demonstrates, space militarization has been with us a long time. It existed as a concept before there was any Sputnik, and since then as an important element in a variety of U.S. and Soviet space programs. Stares reviews the major program initiatives taken by the U.S. as it sought to utilize space to enhance its national security in the postwar era. He surveys a vast array of technological, organizational, and diplomatic activities related to space militarization primarily by marshalling splinters of information from other scholarly writings and from published reports and U.S. government documents, including congressional testimony. He has also uncovered interesting material from first-person interviews and especially from various presidential libraries.

Stares discusses the many forms that space militarization and arms control have taken. However, after all the mass of detail, one hungers for the author to show how the various programs fit into some meaningful pattern, or at least where the major points of congruence or conflict are among all the assorted initiatives and programs.

Overall, though, Stares does a valuable job of assembling the heretofore dispersed and even publicly unknown elements of the story of U.S. space militarization, and does so in a clear style. This makes the book a good contribution to our understanding of what the U.S. has attempted and accomplished in terms of space militarization.

The book appears at a crucial time in the development of U.S. space policy. Although political scientists devoted much analytical attention to space policy during the early days of the space program, their interest waned concurrently with that of the public. One consequence of this drop in social scientific analysis is that space policy has been allowed to evolve without benefit of the careful and constructive scrutiny of the political science community that has done so much to improve design and implementation in other areas of national security policy. This situation is becoming rectified, however, since President Reagan's "Star Wars" speech.

An important conclusion reached by the author is that the absence of space weaponry has not been due to some farsighted restraint on the part of either of the superpowers, but to technical constraints and perceived national interests that until recently had held back the use of space as a new battleground. This is now changing. Today the superpowers—especially the U.S.—are heavily reliant on space. Reconnaissance and communications satellites, as well as space-transiting ballistic missiles, have become standard, crucial lynchpins in their defense planning. These stable assets are now presenting an inviting target due to recent advances in technology that make it possible to contemplate an attack on them. The shortsighted temptation to seek unilateral advantage in these new technologies is proving irresistible.

To the extent that bureaucratic politics and national policy decision making are discussed by Stares, he suggests that far too often in the U.S. there is a tendency to think about space weapons development apart from the context of its long-term impact on the stability and utility of space operations.

Stares has done a thorough job of pulling together information on space militarization. He has also provided thoughtful analyses of the regrettable impact that accelerating space militarization is having on the prospects of arms control.

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China, Taiwan, and the Offshort Islands—Together with an Implication for Outer Mongolia and Sino-Soviet Relations. By Thomas E. Stolper. (Armonk, NY: M. E. Sharpe, 1985. Pp. xiii + 170. $30.00.)

The China Question: Essays on Current Relations between Mainland China and Taiwan. Edited by Yu San Wang. (New York: Praeger Publishers, 1985. Pp. xvi + 164. $35.95.)

Over the last 15 years, Sino-American diplomacy has succeeded at transforming a once uniformly hostile relationship into an economic and, in some ways, even strategic partnership. However, the Taiwan question has persisted as a nagging reminder of the burdens of historic interests. When diplomatic relations between Washington and Beijing were established in 1979, disagreements over Taiwan were temporarily put on the back burner. Yet they keep emerging, and, as these two studies emphasize, they point up the inherent crisis potential of divided states.

Chinese leaders in the People's Republic of China and Taiwan agree on two things about the island: that it is part of China, and that efforts by outsiders to separate it from the mainland must be resisted. Fundamentally, that is what all three Taiwan Strait crises (1954-55, 1958, and 1962) have been about: the status of Taiwan. Thomas Stolper's impressive study of the first affair drives home the view of the People's Republic, from which it has never deviated, that any attempt to give independent status to Taiwan would be rejected, even to the point of using force. Professor Wang's volume is largely a defense and reassertion of Taipei's case to represent the real China. For Taipei, too—and therefore for the book's contributors—an independent Taiwan (or Formosa) is unthinkable.

Stolper's work is the more interesting of the two because it breaks new ground, both as a case study and as an analysis of Chinese irredentism. Meticulously researched and documented, it is the first full-length treatment of Chinese strategic diplomacy in the first confrontation regarding the offshore islands. He finds that since Taiwan, and not Quemoy or Matsu, was Beijing's primary concern, "the PRC did not wish either to capture the island[s] or to have the Nationalists withdraw [from them]" (p. 84). Beijing would behave similarly in 1958. In both instances it had legitimate fears of a U.S.-backed attack on the mainland that led it to make a preemptive strike against well-fortified Nationalist positions in the offshore island group. Capturing the islands was another, preeminently political, matter. It would have facilitated an American claim of Taiwan's independence as an island separate from the others. Mao preferred that the offshore islands continue to be a "noose" around the Americans' neck.

That they no longer are is one of the important features of current U.S.-China relations. The diplomacy of the People's Republic in recent years has again shown that Taiwan is the prize. Various inducements for reunification have been communicated to Taipei. Although, as a matter of principle, Beijing reserves the right to use force to "liberate" Taiwan, it is now pledged to "peaceful reunification" as a "fundamental policy." Stolper cites only a few remote contingencies under which an attack by the People's Republic on Taiwan is conceivable (pp. 140-41). He goes on, however, to observe that the U.S. commitment to Taiwan's sovereignty under the 1979 Taiwan Relations Act lays the basis for a future crisis. So would a unilateral Nationalist withdrawal from Quemoy and Matsu. As always, U.S. policy is the decisive factor, and today, as in past years, it is riddled with contradictions—how else can a "one-China" policy that upholds the right to defend Taiwan be described?—that dismay Beijing and Taipei alike.

If there is a way out of this impasse, Wang's book does not offer one. It is useful in establishing that, even though Taiwan is losing international standing, it still acts very much like a state, and a prosperous one at that, with admirers on the mainland. Most of the authors are too busy pressing Taiwan's case—convinced, in the editor's words, that "the PRC's only course of action is to change the Communist system" (p. 45)—to consider alternatives to all-or-nothing solutions. The case for Taiwanese independence is not considered, for instance. Nor are the implications of Taiwan's serious human-rights problems addressed. The People's Republic's own ideas of Taiwan's future as a special administrative region are mentioned, but not given the serious discussion they deserve. We are left having to believe the Taiwan issue will be with us for many years to come.

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Visions of World Order: Between State Power and Human Justice. By Julius Stone. (Baltimore: The Johns Hopkins University Press, 1984. Pp. xxix. + 246. $26.50.)

This book, Julius Stone's last before his death in the summer of 1985, has two principal aims. The first is to investigate the connection between international law and its "sociological substratum," an investigation that for Stone centers on current technological, economic, and military developments. The second aim is to consider the adequacy of international law as a means of realizing the demands of justice. The focus here is on international law as an instrument for the promotion of human rights and economic welfare. The work thus seeks to contribute both to the theory of international law and to the discussion of contemporary global problems.

Sociological inquiry, the author argues, is essential for the clarification and development of international law. The main task of a sociological jurisprudence of international law is to understand how the law is conditioned by its social environment, for these conditions define the limits within which a world order based on law is possible. One of the most important of these limiting conditions is the difficulty of communicating across religious, class, and national boundaries—a difficulty sometimes exacerbated rather than eased by developments like increased literacy or the spread of television. For example, better communication within a society may create obstacles to communication across national boundaries. According to Stone, the chief defect of most of the major theories of international law—Jenks's hoped-for "common law of mankind," McDougal's "policy-oriented jurisprudence," or Falk's theory of world order through the growth of "planetary consciousness"—is that they are insufficiently attentive to such sociological realities. By thus insisting on the overriding importance of the social environment of international law, the author continues the attack on legalism begun many years before in his *Legal Controls of International Conflict* and other books. The attack is directed here, however, against fellow soldiers in the sociological camp.

To these theoretical concerns is joined a practical interest in reforming international law. In the tradition of ethical jurisprudence, Stone seeks to articulate a conception of justice to be realized through international law and on the basis of which the law is to be evaluated. Justice, according to the view developed here, has little to do with the traditional principles of the law of nations, which in the past required respect for state sovereignty, territorial boundaries, diplomatic immunities, treaties, and the laws of war. On the contrary, the author's focus is almost entirely on matters, such as individual rights and economic welfare, that had been largely outside the scope of international law down to the present century. What the reader will encounter, therefore, are the following distinctly contemporary arguments: first, that international law must secure respect for human rights, and therefore can no longer confine itself to the regulation of interstate relations; second, that it must concern itself, with the promotion of economic justice. Justice, moreover, requires that international law move as expeditiously as it can toward requiring the redistribution of economic benefits to all of humanity, and that this redistribution take place *through* and not merely *to* states.

Stone's ideas regarding the sociological and ethical bases of international law are familiar, and they conform to a large body of current opinion. This superficial plausibility is, however, no proof of their validity. The book is disappointing from the standpoint of theory, because it largely asserts rather than attempting to demonstrate its conclusions. The proposition that in order to understand international law one must study its sociological context seems plausible enough at first sight, but why limit our inquiry in this way? What about the economic context? The psychology of statesmen and diplomats? The historical antecedents of the present order? Does the sociology of international law subsume these other inquiries? Can any such inquiry really illuminate the character of legal order? In a work of theory one expects questions of this kind to be addressed systematically. Stone not only ignores them, but fails to defend his central claim regarding the unique validity of the sociological approach.

The discussion of justice is equally superficial and dogmatic. The author offers a number of propositions about the nature of justice and its relation to international legal order, but he does not attempt to defend them against even the most obvious objections.

Clearly this is unsatisfactory, given both the controversial character of the concept of justice and the novelty of Stone's own position in the history of discourse regarding justice in international society.

This is an earnest essay, but it contributes little to the present controversy over the nature and moral foundations of international legal order. Those who wish to follow Stone's work will find here a restatement of many of the themes that occupied him during a long career. Others should look elsewhere for significant discussion of the problems of international justice.

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Peacetime Unilateral Remedies: An Analysis of Countermeasures. By Elisabeth Zoller. (New York: Transnational Publishers, Inc., 1984. Pp. xviii + 189. $35.00.)

This book deals with the important subject of the extent to which states can take the law into their own hands when their interests have been thwarted by other countries. Professor Zoller explores the subject of peacetime unilateral remedies in every legal way conceivably possible, determining which are acceptable and the conditions that make them possible. The book would be of greater interest to international relations specialists had it included case studies explaining why states will carry out illegal unilateral remedies knowing they are against the law.

The first part of the book carefully distinguishes between the countermeasures of retortion, reciprocity, suspension and termination of a treaty, and reprisals. On retortion, the author feels that most of the acts are done under domestic jurisdiction and thus are legal. She observes that the United States practices retortion a great deal, giving the examples of the Hickenlooper Amendment on compensation problems of nationalized overseas investment and human rights provisions in legislative acts. In both cases aid to other nations could be denied. Professor Zoller mentions that the intention of retortion should be considered, and that an economic boycott could be unreasonable. The United States economic embargo on Nicaragua, which occurred after the book was written, would be an excellent case to include, since its legality was questioned by many countries in a United Nations Security Council debate on the issue.

The next countermeasure, reciprocity, is described exceptionally well, with excellent examples of the General Agreement on Tariffs and Trade (GATT) system used to show that as long as a state stays within equivalence the concept is legal. The next countermeasure, suspension of a treaty, is evaluated largely from the provisions of the Vienna Convention on Treaties. Finally, there is a long section on reprisals, covering their origins and the viewpoint that Article 2(4) of the United Nations Charter prohibiting the threat or use of force makes most of them illegal. This section on reprisals could have been made more pertinent to international relations if there had been an explanation of why so many states in the postwar period have used reprisals, regardless of their illegality. The Israel reprisals against the PLO could have been evaluated here.

The first part of the book concludes with a chapter on the specificity of countermeasures. Professor Zoller here is interested in getting at the purposes of unilateral remedies. She does this through a threefold classification scheme of reparation, coercion, and punishment. By describing what is involved in each countermeasure and the motives of states in selecting one or the other, the author is able to assess the legality of each. For reparation, the ideal situation would be a return to the *status quo ante*, but since this is never truly possible, monetary restitution is the most acceptable legal procedure. Coercion is defined as coercively affecting the future behavior of the other party in a nonviolent way, and is considered by the author to have a utilitarian purpose of law enforcement. Punishment, however, is considered to be illegal, since its purpose is the infliction of harm, the use of excessive force, the thirst for revenge, and the demonstration of superiority. It also lasts much longer than necessary.

The second part continues with a discussion of the legal framework of countermeasures. In one part the author argues that it is legally prohibitive to take countermeasures if they harm third states, although the author feels that it is worth determining if the act is done purposely or unintentionally. Here an account of Israel's reprisals against the PLO in Lebanon could have added to the analysis. In another interesting part, the author feels that there is no legal responsibility to negotiate before committing reprisals, since negotiations could be an odious trap set by the other side to allow the dispute to drag on and on.

Professor Zoller concludes the book by making a strong plea for the acceptability of certain types of countermeasures, as long as all of the legal conditions described so carefully in the book are met. An excellent twelve-page bibliography of relevant books, articles, and court cases is included.

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POLITICAL ECONOMY

Economic Conditions and Electoral Outcomes: The United States and Western Europe. Edited by Heinz Eulau and Michael S. Lewis-Beck. (New York: Agathon Press, 1985. Pp. viii + 248. $15.00, paper.)

Lewis-Beck organized the University of Iowa's Shambaugh Conference in March 1984, at which these papers were presented; Eulau edited them for *Political Behavior*, where they first appeared. The editors provide a well-rounded introductory essay, setting the stage for the nascent cross-national comparisons inhering in these reports. But do we really need another reader on this topic? Is the whole greater than the sum of its parts?

The volume succeeds admirably by illuminating two important questions: (1) How does the context of elections condition the economy's effect on the vote? (2) What causal process (i.e., what perceptions and decisions by individuals) produces the link? "Context" includes social, economic, or political factors that shape popular political responses to economic events. Social factors, for instance, include cultural beliefs in self-reliance vs. group or national solidarity; economic factors, the country's openness to international trade or monetary conditions beyond the control of national leaders; political factors, the many types of rules affecting the visibility of policy or the ability of citizens to hold governments accountable—majority vs. coalition governments, large vs. small public sector, or "strong" party systems (cleavage lines are simple and ideologically clear, and virtually the whole population is mobilized) vs. "weak" ones. Assessing the impact of these influences requires cross-national comparisons, and these rely on having at hand the appropriate information about numerous countries.

Schneider provides an initial overview of the literature, noting some salient differences in research traditions and findings among the countries. Except for Germany, most Western European nations receive article-length treatment, with a review of research and a brief introduction to the author's latest statistical analysis of the economic sources of electoral outcomes. Whitley focuses on Britain's 1983 election, an appropriate research site, as the class-based party alignment was then at its weakest and the issue of economic policy under Thatcher presumably highly salient. Lafay's analysis centers on the French election of 1981, contrasting the covariation between economic conditions and executive popularity under Giscard with that under the Mitterand government's first two years. Miller and Listhaug summarize the voluminous literature on economic voting in the Scandinavian nations and present a perceptive and suggestive study of Norwegian voting. Bellucci's study of Italian elections since 1980 depicts a system in which the strongly ideological nature of party competition makes short-term economic issues largely irrelevant. And Lancaster's study of Spain since the transition to democracy argues that unemployment has only recently emerged as an issue, both because of the moderation of the post-Franco left and because of the early "politics of consensus" on the primacy of building democratic legitimacy. Norpoth's study of U.S. presidential popularity from 1960 to 1980 belongs to this category. A tour de force of ARIMA time-series analysis, its results—neither Kennedy nor Johnson benefit from their management of the economy, while Ford does, according to the model—will convert few skeptics to the method. These studies map the effects of several important contextual variables: they should provide valuable raw material and strong stimulus to explicitly comparative research.

The task of explaining how the political behavior of individuals gives rise to observed aggregate covariation between the economy and elections is a difficult one. While survey research can clear up some of the causal ambiguity inherent in time-series analysis, its own imperfections can be narrowed only by learning how citizens translate objective economic experiences into subjective, politically meaningful understandings. Kiewiet and Rivers review the research on economic voting in the U.S. with a view to contrasting the two approaches, and they provide both a perceptive summary of current knowledge and several promising suggestions for future research. Much of the individual-level research has addressed the question of how economic self-interest guides the vote. Feldman ranges widely over this literature, judiciously evaluating its worth and drawing out (and testing) a number of critical hypotheses about the interplay between personal financial condition and national business conditions as alternative decision referents. Peffley continues this theme, considering several models of the reasoning process through which individuals attribute political significance to economic events. A brief essay by Toinet, suggesting future research directions, concludes the volume.

The editors set out to explain how economic conditions influence elections, but they apparently are not comfortable omitting the other side of the question—How do elections influence economic policies?—and Alt's article compares British and American experience. From a sophisticated specification of the policy effects of party change within an economic model including international influences, Alt infers that party differences result in transitory, not sustained, shifts of the unemployment rate. Curiously enough, changes in party control cause distinct economic outcomes, but without corresponding differences in policy instrument settings, underlining the fact that the policy side of the question is at least as complex as the election side. Pfiffner has recently edited a comparable volume focusing on these issues.

It is no failing that this collection covers less than the whole field: it takes on a significant portion and treats relevant questions thoroughly, with subtlety and some originality. It is a failing that a host of typographical and proofreading errors mar the text, but with this exception, the volume is a fine investment for the growing number of upper division and graduate courses in political economy.

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Government Finance in Developing Countries. By Richard Goode. (Washington, D.C.: Brookings Institution, 1984. Pp. 334. $31.95, cloth; $13.95 pages.)

"Throughout the world," asserts Richard Goode, "many government declarations of intentions are never carried out. Other intentions are partly realized but at high cost" (p. 301). His book, *Government Finance in Developing Countries*, is addressed to the question of how this gap between promise and performance in developing countries may be narrowed through the adoption and carrying out of practical and appropriate financial policies. It aims to provide a comprehensive survey of current theories and practices in budgeting, expenditure analysis, taxation, borrowing, and financing government by money creation, and to examine these topics in relation to institutions and problems in developing countries. The result is a detailed handbook combining prescription and description in the light of the author's 30 years of experience with the International Monetary Fund and the Brookings Institution.

Goode's primary approach is that of the familiar public finance text, setting out and discussing accepted principles in the light of advantages and disadvantages. The particular value of his book is that he goes much further, relating those principles to conditions in developing countries, citing research and experience to illustrate their potential and limitations. Nor are the principles themselves immune to criticism—such textbook staples as public goods theory and the concept of Pareto efficiency, for example, are dismissed in a few words as having little practical bearing on the issues at hand, which are the feasibility and usefulness of principles translated into policies.

What, for example, are the prospects for an income tax? Well, although conditions according to the literature would seem to rule it out, there is nothing to stop you trying it and gradually extending its coverage, but don't rely on it as a major revenue source, and if you set beginning rates too low and top rates too high, you have only yourselves to blame, because the hazards of both are spelled out for you. What about a land tax? Or a consumption tax? What are the effects of export taxes? How about capital budgets? For these and a wide variety of financial policy questions Richard Goode carefully sets out the pros and cons, summarizes the research, comments (often critically) on current practices, provides references for further study, and gives cautious advice.

It is quite clear that there are no simple answers. Rather, the answers for each country have to be worked out through a maze of possibilities, economic constraints, political limitations, and value judgments. Criteria lie in effectiveness, (the degree to which intentions are realized) and efficiency (the extent to which they are attained at reasonable cost in scarce resources or sacrifice of other objectives).

This pragmatic approach has its limitations. Financial policies are discussed one at a time, with little attempt to relate them one to another. There is no attempt at a systematic conception of developing countries that goes beyond seeing their difficulties as simply one more example of world-wide fiscal strain. Nor does the author try to distinguish among developing countries in order to relate fiscal policies to specific sets of circumstances. For example, a table of countries showing relative tax effort is presented, but no explanations or conclusions are given regarding why some countries exhibit lower tax effort than others. Generalized criticisms of current practices are often offered, but then the author hurries on to another topic, providing little or no insight into why things are as they are. Similarly, despite a promise at the outset to include political institutions and administrative capabilities in the analysis, these are really treated as just another set of constraints, and there is no systematic attempt to relate the nature of regimes to financial policies or practices.

Yet the book is still an invaluable practical reference compendium that should be essential reading for anyone concerned with policy making in developing countries. While it concludes that poverty is the major cause of policy ineffectiveness and that financial problems are likely to remain intractable, it emphasizes the degree of choice realistically open to policy makers in adapting the systems they inherit, and the practical possibilities for overcoming or compensating their handicaps. Some countries do much better than others. Why? If there is a single broad maxim generally applicable, it is to economize in the use of scarce resources, and there is much practical advice as to how this may be done. If the book seems unsatisfying because of its generalized approach, the Brookings Institution might well consider publishing a collection of case studies to supplement it and stimulate in-depth discussion of the issues it raises. Meanwhile Goode has provided an easily read and useful handbook for all concerned with financial policies, not just those in developing countries.

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The Politics of U.S. International Trade: Protection, Expansion and Escape. Stephanie Ann Lenway. (Marschfield, MA: Pitman Publishing, 1985. Pp. xii + 228. $25.95.)

In this fifth work in the Pitman Series in Business and Public Policy, Stephanie Lenway compares four explanations of U.S. trade policy outputs through a close examination of three highly visible cases. She concludes that only by way of continued U.S. participation in the institutional framework provided by the General Agreement on Tariffs and Trade (GATT) will U.S. national interest prevail over protectionist sectoral interests.

The author begins by identifying four possibly complementary explanations. First, the interest-group explanation portrays U.S. trade policy as the output of a Congress captured by various domestic business and labor interests. Second, a transactional explanation suggests that Congress will act autonomously by removing itself from a direct role in trade policy formulation and only acting as a final arbiter. Third, a state autonomy explanation would see the U.S. as attempting to improve its relative international power through policies to increase its exports as compared with its imports. The author finds each of these inadequate as direct explanations of the pragmatic liberalism consistently identified in U.S. trade policy.

Lenway argues for a fourth explanation, in which trade policy reflects a broad national interest in economic liberalism. Even though U.S. trade policy does aid special interests and maintains domestic social stability, the existence of international norms and rules, as embodied in the GATT, allows the state to resist domestic protectionist sentiment.

Through a careful study of the U.S. stance on the multifiber arrangements associated with the Tokyo Round multilateral trade negotiations, the U.S. response to the increase in Japanese automobile imports, and the U.S. position on the inclusion of the Japanese telecommunications market in the Tokyo Round negotiations on the Government Procurement Code, the author tries to find the links implied in each explanation. Lenway finds that, although none of the explanations failed completely and all are to a degree complementary, "all three cases were consistent in that the final decision involved adherence to the norms, rules, and procedures of the multilateral trade regime" (p. 205).

The literature review succinctly examines the domestic connections of U.S. trade policy, and though the cases are common to the literature, the mix of evidence from interviews, documents, and scholarly works provides a very detailed description of the process. More importantly, the focus on the four explanations keeps the cases in perspective, permitting a fair degree of comparison in the analysis. The literature review and policy descriptions are well-styled, though occasionally redundant, making the work very readable. Given that path-breaking works on trade policy have laid the basis for some of the most influential models of American politics, this alone gives the work some appeal.

However, the cases most clearly demonstrate that the U.S. also used domestic pressures to carve exceptions from the international norm of liberalism, by emphasizing the strength of domestic protectionist demands being kept at bay (pp. 102, 126, 157). This implies that international restraints on U.S. policy were affected by domestic demands, which to some degree reverses the international constraints explanation. Furthermore, the author argues that the imposition of voluntary export restraints and the construction of a restrictively ordered market under the multifiber arrangement is consistent with the norm of liberalization, a very confusing interpretation of the norms of the international trade regime. Pragmatic liberalism may be standard behavior, but it is not the same as the international standard of liberal behavior.

Finally, though the author specifically avoids using the terms *hypothesis* and *theory*, the interest-group model, the state autonomy model, and the international constraints model are generalizable explanations for trade policy behavior, and a comparative perspective would be of even greater value. Nonetheless, this book is a useful complement to other, more comparative, materials on the domestic determinants of trade policy.

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Game Theoretic Analysis of Voting in Committees. By Bezalel Peleg. (Cambridge: Cambridge University Press, 1984. Pp. 144. $37.50, cloth; $13.95, paper.)

Can a voting system be devised that is guaranteed to reveal voters' true preferences and to choose a desirable outcome? It has been proved that in general, the answer is *no*.

Unless voters' preferences are assumed to be restricted (say, to single-peaked preferences in a unidimensional space), there is always the chance that some voters can gain by reporting a false preference ordering. This book investigates how serious this problem may be. It is largely a description of recently published research by Peleg, Moulin, and others.

Peleg's approach is to develop formal representations of voting games, and investigate whether these can be proved to be nonmanipulable. I think it is fair to say that the results are mixed, although much has been learned about specifics. For example, Peleg brings Arrow's impossibility theorem and the Gibbard-Satterthwaite manipulability theorem into his framework and relates them to his results. Peleg emphasizes "strong representations" of committees—social choices that accurately represent the expressed preferences and power of the committee members, and that assure that the members cannot manipulate their expression of preferences to make themselves better off. Most work on manipulability has looked for Nash equilibria, but Peleg investigates the harder and more important problem of whether any coalition of voters can make themselves better off by falsely stating their preferences.

"Strong representations" are available for committees with some distributions of preferences and numbers of alternatives, although the precise conditions are still being worked out. Implementing a strong representation can be done by a high-enough majority rule that depends on the number of voters and alternatives. (This result is much like Mueller's voting by veto). Peleg investigates whether agenda structures make implementation of strong representations easier, and comes to negative conclusions. This supports the Romer-Rosenthal and Shepsle-Weingast approach that sees agendas as a means of manipulating outcomes, not of assuring nonmanipulable social choice.

Another formal book on this topic is Herve Moulin's *The Strategy of Social Choice* (Amsterdam: North-Holland, 1983). Both books are very difficult for readers without an understanding of the formalism of social choice theory. Moulin's book has a number of exercises that are worth working through, and as a textbook provides a broader coverage of the field and more intuition. Peleg concentrates on a narrower set of questions, and provides more proofs.

Some concerns I have with this work include its neglect of incentive schemes of the Clarke-Groves-Ladyard variety, which could reduce misrepresentation. By looking only at rank orderings, this approach neglects cardinal differences in utilities, which might be important in committee decision making. It also excludes binding agreements, side payments, and an external exchange value. These exclusions seem inappropriate for a positive theory based on cooperative solution concepts. Finally, the book's main results favor super-majority rules, which have high decision-making costs that are modeled in the public choice literature. These costs affect the optimal outcome and are inconsistent with the zero coalition costs Peleg assumes.

This book is written for, and will be useful to, social choice theorists interested in the analysis of voting rules.

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Capitalism and Social Democracy. By Adam Przeworski. (New York: Cambridge University Press, 1985. Pp. viii + 269. $39.50.)

This is the second book in a new series, Studies in Marxism and Social Theory, and it can be read as both a contribution to and a critique of the Marxist literature on class struggle and the state. The book is a reworking of several prior articles, some cowritten with John Sprague or Michael Wallerstein. The style ranges from historical narrative to formal modelling, including the use of graphs and regressions. The density of the argument is, happily, leavened by a wry humor in examples and footnotes.

Przeworski's points are both political and methodological. Politically, he argues that social democratic struggles are correctly viewed as being focused on material issues within capitalism rather than as stepping stones toward a socialist transformation. Unlike many Marxist critics of social democracy, he does not think this is necessarily a bad thing. It may be rational for workers and their political parties to struggle for these reforms, rather than face a long period of sacrifices to get to some still-unclear noncapitalist society. Przeworski is not overly optimistic about what can be gained through social democracy, particularly in a period in which Keynesian policies for demand-side growth have little room to operate, but he sees social democracy as preferable to an unorganized alternative. While not unsympathetic to the separate project of a socialist transformation, he sees it as not immediately feasible.

Methodologically, Przeworski is concerned with developing a theory of politics and class behavior based on a formal rationality model. Thus, after his general overview of the history of social democratic parties and their relation to the broader socialist movement (chap. 1), and a history of the debate over class and its formation (chap. 2), he turns to modelling the material basis of class and party organization and strategy. Late in chapter 2 and in its appendix, he argues that not merely class consciousness and cohesion, but the very division of the population into classes, must be explained on a model of individual choices. He gives a provocative example of a woman who may, depending on a variety of factors and choices, end up as a housewife, a factory worker, or in a very small business. "Her objectives and her resources do not classify her as a worker," although he might add they clearly keep her out of the big bourgeoisie. "She decides to become a worker given her objectives and resources" (p. 95). Thus class boundaries as well as class strategy are to be explained. Class analysis becomes "a methodological postulate . . . that all conflicts that occur at any moment of history can be understood if and only if they are viewed as effects of and in turn having an effect upon class formation" (p. 80).

In chapter 3, Przeworski argues that once the socialist and labor movements had begun to organize workers as a class, and move toward class power in a political arena, the parties they created were faced with a choice between broadening their program to attract non-factory workers as voters or narrowing their program. This created tradeoffs. Too much broadening would cost worker support. The theoretical optimal degree of broadening, which might or might not be reached exactly, did not include possibilities of an immediate drive for socialist transformation. Similarly, working within capitalism, there was an optimal degree of militancy to obtain the highest economic gains. Too much militancy would lead to economic crisis and would thus be as bad as too little. The result was to make class compromise a rational choice for both workers and their parties. (chaps. 4 and 5).

The final technical analysis, chapter 7, a critique of a paper by Roemer, discusses possible rational arguments for socialism, arguing that the case can better be made in terms of increased possibilities for social choice, rather than on purely economic grounds. On either side of this chapter, chapter 6 and a postscript draw political conclusions for the current period. With Keynesianism unable to satisfy both workers and capitalists, the tradeoffs are worse, but it is in the workers' interest to remain organized as a class, despite the difficulties. Socialism in the long run, and social democracy in the short, both appear as desirable, but they are entirely separate objectives.

Students of rationality models will find this book an interesting application. Marxists may object to this particular revisionism, and to the individual-rationality methodology itself, but they should still consider the arguments. There is no inherent incompatibility between materialism at the micro and macro levels, although specific models of their relationship may be too narrow. Certainly a reanalysis of the history of class compromise, a theme recently raised in studies of housing policy and workplace control, as well as of national politics, is due. Przeworski may be faulted on a number of points, particularly for taking the nation as a given unit of analysis when he is going beyond that for class as a unit, and for underplaying the economic forces that erode the payoffs to compromise. His arguments for the methodological attempt are, however, worthwhile.

MATTHEW EDEL

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How the West Grew Rich: The Economic Transformation of the Industrial World. By Nathan Rosenberg and L. E. Birdzell, Jr. (New York: Basic Books, Inc., 1986. Pp. xii + 353. $19.95.)

A distinguished scholar of the history of technology and a legal specialist have teamed up to write still another book on the rise of the West. In *How the West Grew Rich,* Nathan Rosenberg and L. E. Birdzell explore the Middle Ages, the growth of trade, and the evolution of institutions favoring commerce. They look at the development of industry between 1750 and 1850, at technology and organization, at the links between science and wealth, and at organization in terms of the diversity of enterprise, and conclude with a chapter on implications and comparisons.

The central theme of the book is that the rise of the West can be explained by decentralization. That is, there was a gradual evolution of a set of institutions that permitted the economic sphere to become autonomous from the political-military sphere. The development of favorable institutions promoted a freedom to experiment in developing new forms of organization and of innovation. In addition, the development of a morality on the part of those who engaged in economic activity induced trust and contract fulfillment. The book is written in nontechnical language and contains no explicit theorizing; therefore, it is well suited for a general audience.

It is hard to quarrel with the author's argument. It is not a new one. The notion is that the evolving, competitive structure of western economies and the gradual emancipation of the economic sphere from the politics of mercantilism permitted and encouraged rapid economic growth. A long line of authors have in one way or another made this point, and most of them are the building blocks for *How the West Grew Rich.*

In the early part of their story, Rosenberg and Birdzell's emphasis is, of necessity, on the Netherlands and England, since other parts of Western Europe don't really fit it very well. France, Spain, and Portugal not only fail to develop and provide the institutional framework, but the latter two actually stagnate over the next centuries, and only catch up with the western world in very modern times (and then not completely). Here we have a fundamental issue. In every country, rulers wanted to get their hands on the growing wealth and income of societies as they evolved. Indeed in France and in Spain they succeeded with consequences that were deleterious to the growth of their economies. In the Netherlands and England, on the other hand, the gradual development of an economy that had wide latitude to experiment and whose competitive conditions produced institutions favorable to growth, yielded a completely different pattern. Why was this so? Obviously the underlying game is between the polity and the economy. While the description of the two authors here is fine, it is not an explanation. They do refer to the growing mobility of capital as a deterrent to ruler confiscation, and cite the contrasting policies of the English and the French, but we are not provided with a body of political, political economic, or other theory about how institutions evolve that accounts for this diverse pattern between England and the Netherlands on the one hand and France and Spain on the other.

The same problem holds with respect to the authors' emphasis on the importance of the development of morality and a standard of ethics among merchants, which helped to enforce and lower the costs of contracting. What produced such development, and in what context? They flirt with the Protestant Revolution as an explanation (p. 114), but leave the issue in doubt. Clearly, the increasingly effective enforcement of contracts, whether as a result of morality or of enforceability, is an important factor in the development of markets that require "credible commitment."

A major strength of the book is its emphasis on the importance of institutions and its recognition of the interrelationship between favorable institutions and the development of experimentation and innovation. If it lacks any explicit theory, it is nevertheless rich in description of the institutions and, as is to be expected from one of the world's leading scholars in the history of technology, in discussion of technological change.

For the political scientist this book certainly provides an interesting story, but one that must, at least in part, be disappointing, since there is neither political theorizing nor any integrating of political explanation with that of the evolution of economies. An interesting few pages in the last chapter on the political sphere (chap. 10) say some very sensible things about the separation in former times and nonseparation in modern times of polities and economies, but the story is not told in the context of serious analysis.

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Does Politics Matter? The Determinants of Public Policy. By L. J. Sharpe and K. Newton. (New York: Oxford University Press, 1984. Pp. xviii + 249. $19.50.)

The topic of output studies evokes a strong sense of deja vu—one of those fashions in political science now relegated to the recesses of memory. Sharpe and Newton revisit this child of the early 1970s, demonstrating that premature abandonment of the new baby can be as unrewarding as its enthusiastic and ill-considered adoption.

As ever, the conventional approaches to explaining why policies differ are critically reviewed and found wanting. In their place—the core of the book—the authors demonstrate the superiority of the party and party system model and the unitary model. The former employs a wider range of measures of party effect on disaggregated expenditures (over time). Most important, the analysis distinguishes between party control and type of party system—e.g., non-party, two-party—demonstrating that both influence expenditures. The unitary model demonstrates the effect on expenditure of city type, or differences in economic base and function, and of the effect of a city's place in the urban hierarchy.

Perhaps the most appealing feature of the book is the common sense that pervades the analysis. It relies on relatively simple statistical techniques and insists on linking techniques to a well-defined theory and associated propositions. Rather than reaching for their computer terminal, the authors were prepared to inspect simple "league tables" of service expenditure and point out, for example, that local authorities on the coast spend less on sewerage. The case for the importance of politics is made with equal clarity and directness. Given Jim Sharpe and Ken Newton's high standing among students of urban politics in Britain, one would expect no less. The problems do not lie in how the study is executed, but in its terms of reference: the theoretical and methodological rationale for output studies.

The academic controversy over the relative importance of economic and political variables is seen as a non-contest. The authors accord the political process "its true primordial status," arguing that "the political system is to public policy what an individual's perceptual set is to his cognition: it abstracts some features of the social and economic environment and gives them political salience" (p. 207). Additionally, "it also extends out into its own environment in order to shape, influence and alter it" (p. 208). As a result, key determinants of public policy "are almost certainly not susceptible to output analysis," most notable "those political factors that play a part in policy formation" (p. 216). Numbered among these factors are the role of the bureaucracy, the role of leadership cadres among the elected representatives, and a polity's decision-making tradition (p. 217). The authors call, therefore, for an alliance of methodologies. Output studies are to form the basis for future case studies of political factors. They will facilitate the precise design of case studies—for example, the selection of cases would be guided by the author's typology of local authorities—and "the inferences we have made from the macro data about the principal determinants of policy may be put to a searching and detailed test" (p. 216).

In thus rejecting the typical "input-black-box-output" paradigm in favor of the primordial political system, the authors undermine the theoretical and methodological rationale of the very approach they seek to rehabilitate: the environment is no longer constitutive, but constituted. To abandon the core model of output studies, to reverse the causal sequence, to redefine political variables and assert their primacy, and to discount the methodology of output studies for studying key independent variables is not to revise a theory or approach, but to abandon it. Although the authors enter a plea for cross-national output studies (which include subnational governments), they emphasize the contribution of case studies. There are no attempts to develop indicators of bureaucratic power. For example, it could be argued that the career mobility of officials was related to their influence, with mobile officers seeking to promote policies to further their future advancement. Consequently, it would seem that the authors also see output studies as a cul-de-sac, a tool for providing a systematic sketch map of the terrain to be traversed, but incapable of providing anything but clues for a more detailed analysis of policy. Politics does matter. Unfortunately, output studies do not. The way forward lies with different theories and methodologies.

Sharpe and Newton's research of times past demonstrates how output studies ought to have been conducted, but it does not demonstrate why this approach remains of value for future studies of policy making. Indeed, they are often their own best critics, with their clear appreciation of the limits to output studies. Thus, after criticizing previous attempts to operationalize political variables, they provide their own, more all-embracing indicators, which they then criticize by listing political variables not amenable to numeric expression. As ever, rehabilitation is a grand ideal applied to base material. It is to be hoped that recidivism rates do not increase. Yet more output studies, however well executed, would again direct energies and attention away from the analysis of policy, its formation, and consequences.

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Exploring the Global Economy. By Raymond Vernon. (Lanham, MD: United Press of America, 1985. Pp. vii + 225. $19.95, cloth; $8.75, paper.)

The Politics of East-West Trade. Edited by Gordon B. Smith. (Boulder, CO: Westview Press, 1984. Pp. ix + 267. $32.50.)

Few economists have had the influence of Raymond Vernon on the way political scientists understand issues of trade and investment. In this compilation of Vernon's latest essays, he reexamines many of the key ideas associated with his earlier works. Addressed are four topics: international trade, international investment, the state-owned enterprise, and trade with the Soviet Union. In latter two sections the work of the economist speaks directly to the set of political issues discussed by the authors in Gordon Smith's collection.

Most fundamentally, Vernon's essays indicate the extent to which the international economy has changed in the 15 years since he published *Sovereignty at Bay*. In 1971, Vernon questioned the viability of the nation-state system; in the 1980s, Vernon's concerns have shifted to the future of the liberal trade regime. Focusing on the growing number of transactions no longer conducted under the General Agreement on Tariffs and Trade (GATT), Vernon's essays examine their implications for states, for multinational enterprises (MNEs), and for the future of the international economic regime.

In an opening essay, Vernon summarizes his position: "The institutions of international trade commonly march to a music that was not written for the GATT script" (p. 19). He argues that a growing proportion of the world's trade is conducted on a basis considerably at variance with the principles that have characterized postwar trade among western nations. The existence of MNEs, state-owned corporations, countertrade arrangements, export-restraint agreements, and bilateral state trading with the Eastern bloc all undermine the rules, norms, and principles that characterize postwar economic arrangements. Domestic pressures for protectionism are not the only threats to the trade regime. In an essay on the future role technology will play in trade relations, Vernon hypothesizes that technical information and training could become a substitute for the shipment of goods. Technology itself, then, will decrease levels of world trade. As opposed to earlier analyses, in which he viewed information as a stimulant to trade, Vernon now sees a world in which information substitutes for trade and innovation places "greater stress on cost-reducing changes and lesser stress on the satisfaction of new wants" (p. 30).

Of the problems confronting the liberal economic order, multinational enterprises remain a key variable to the form future trade relations will take. The MNE, however, looks a lot less formidable in these essays. Although national governments have not been able to coordinate a uniform set of rights and norms for international companies, individual corporations face considerably more opposition to foreign direct investment than they did in the 1970s. Vernon argues that the convergence of an "obsolescent bargain" and reemerging nationalism fueled governments to reassert their authority over these corporations.

One alternative pursued by nation-states has been to enter the market as corporate owners. Turning to the rise of state-owned corporations, Vernon examines the international implications of these companies. "The substantial growth of state-owned enterprises might inhibit the expansion of MNEs" (p. 110), he argues. More probably, however, the two will strive for mutual adaptation. In replacing the MNE, states face a number of constraints. As domestic champions, they will have a propensity to use domestic inputs and strive for export earnings, even if this is not the optimal economic strategy. Since these companies cannot invest abroad to rationalize the costs of production, they tend to be less flexible and will not produce at economies of scale. State ownership also brings a myriad of management problems: "Governments have great difficulty in defining their purpose in a coherent, internally consistent manner" (p. 151). Within any polity, there will be rival perspectives on the purposes of corporate ownership. Certainly, no state manager has settled for merely dealing with market imperfections. Nations always introduce some set of mixed policies that serve domestic needs: "The state-owned enterprise epitomizes a dilemma that has always existed . . . in a democratic state" (p. 160). Shall it be workers', managers' or ministers' interests that are served by the state when it turns to production? Polities maintain differing balances among these forces that "reflect the values of the country concerned" (pp. 161–62). Vernon concludes, however, that all meet similar "compelling demands that derive from operating in oligopolistic markets" (p. 162).

State ownership among the Eastern nations has led to another set of issues. Vernon sees the current pattern in East-West trade as a threat to the trade regime. Vernon looks to the potential use of commodity power to upset the economic system. He argues the Soviets could do three things: they could use their power to upset the international economy; they could use power to influence the individual behavior of nations, or they could attempt to capture the lion's share of economic gains from ordinary international trade. Vernon concludes that the first is improbable. The Soviets are dominant sellers only in chromite, manganese, and platinum; they are not dominant buyers of any commodity. Even so, Vernon argues that they may be able to influence inflationary and deflationary movements out of proportion to the size of the USSR in the world market. This is not probable, however, because he sees "no such subtlety of purpose" evident in Soviet behavior. The second and third options are more feasible. The Soviets can and probably have affected the behavior of the countries in the Council for Mutual Economic Assistance (CMEA). This, however, is not a threat to other trading partners. What is more evident to Vernon is that the Soviets gain asymmetrically from trade. Why? Vernon argues that since the level and composition of trade is determined by the East—that is, they only trade if there are gains from trade—trade with the West, where private gains may not always equal social gains, leads to a Soviet advantage.

In an essay written with Marshall Goldman, Vernon moves away from issues of market control and turns to prescriptions for American policy. Here, Vernon's work and Smith's work converge. Vernon organizes his discussion of American policy around three basic questions. He asks, "How will restrictions on the export of strategic goods and technologies affect the behavior and performance of the Soviet Union and COMECON [same as CMEA] . . . are there common policies with which (Western) countries can be expected to agree . . . and what economic difference will the presence or absence of restrictions have on the U.S. economy?" (p. 184). Vernon's responses are straightforward. The attempt to restrain trade neither helps curtail the Soviet's war-making capacity nor works as an effective signaling device for American displeasures with Soviet behavior. Vernon argues that restraints may actually be counterproductive; they may serve to stimulate the Soviets to find effective substitutes. Given the history of American policies, Vernon is pessimistic about finding a common ground for cooperation among the Western nations concerning trade to the East. Rather, he argues that current policies will serve to weaken the GATT regime: "Under existing institutions and rules of the game, increased East-West trade will strain the trading relations among the

Western states and will distribute the economic benefits of trade disproportionately to the East" (p. 199). Thus, increased trade under current conditions in which the U.S. attempts unilaterally to mandate restrictions will have a negative impact on the U.S. For Vernon, the alternative is straightforward. "For those who are concerned with maintaining an effective set of economic relations toward the USSR, U.S. efforts to strengthen an open trading system for the world offer the most feasible area for action." (p. 194).

The more empirical studies found in Smith's volume agree, for the most part, with Vernon. *The Politics of East-West Trade* is a compilation of essays by American and European scholars, government officials, and businessmen. Three themes tie these essays together. First, a number of authors address the issue of the degree to which the Soviet economy is dependent on imports to the West. Second, the book looks at the impact of such dependence on the Soviet Union. Third, authors address the reciprocal affects of America's policy on East-West trade.

What is the extent of political leverage the West could garner through trade policy? Five authors examine case studies oriented toward answering this question. John Martens begins with an attempt to model the flow of imports into the Soviet Union. He discovers that the composition of exports to communist countries looks essentially like the general pattern of world-wide exports. Whether because of export controls or because of Soviet initiative, Martens concludes that trade with communist countries has not overly emphasized high technology products. In one of the best essays in the volume, Elizabeth Goldstein traces the impact of technology transfers on a sector in which she expected technology transfer to have a positive influence. In her case study, she finds that the ferrous metals industry suffered low productivity growth and no improvement in quality and yield, even though there were imports of advanced foreign technology. She argues that the combination of a seller's market and a systemic bias against innovation in the Soviet Union leads to disincentives among managers to improve performance. Again, there seems to be neither dependence on nor the ability to diffuse foreign technology.

Three other case studies corroborate Goldstein's findings. Jan Vanous argues that Soviet fear of dependence on the West has a major impact on policy. Even though the Soviets have a substantial disadvantage in grain, relative to energy trade, they refuse to specialize. Dependence based on comparative advantage is politically unacceptable to Soviet leaders. S. E. Goodman, in a study of the computer industry, finds numerous problems with the integration of foreign technology. Goodman however, argues that many foreign companies have had similar problems with technology transfer. The Soviets benefit most from technology that is open; that is, from technical and scientific reports that are part of the public domain and not subject to government control. Further showing the lack of Eastern dependence on Western products, Thane Gustafson examines how the Soviets compensated for the American embargo on products for the gas pipeline. He found that the Soviets were able to build their own compressors through a policy of explicit political and resource targetting. He argues that leadership will pay any price to defend their autonomy. In short, Western embargos lead neither to political leverage nor to the curtailment of essential production. Embargoes do, however, exact a high cost from the Soviet economy. As Smith sums up, "there are tangible limits on the ability of the U.S. to use trade as leverage to alter Soviet policies."

A set of authors argues that there may be tangible costs to the U.S. in restricting trade. In an interesting article on the domestic effects of economic sanctions, H. Stephen Gardner argues that the major cost of restrictions is in the distribution of income. Income is redistributed away from affected sectors. In three other categories he sees the effect of sanctions to be mixed. It is unclear whether there is a substantial balance of payments loss; due to "musical chairs" trade patterns and a decline in exchange rates, the industries he studies had exports more than offset by opportunities in new markets. Similarly, Gardner found that offsetting market forces results in no real losses in employment nor in the allocation of resources due to economic sanctions. Garner does not believe that there is no effect from export controls. Rather he sees the U.S. as "shooting ourselves in the foot" (p. 194). Luckily, there is adjustment. In separate articles, Lothar Griessbach, William McIlvaine, and Gary Bertsch and John Mcintyre argue for the destructive influence of embargoes. In general, the politicization of market transactions leads to a mistrust of American corporations and to distrust of the U.S. among members of the GATT regime.

The lone dissenter arguing for the virtues of trade restraints was Donald Goldstein. Articulating the position of the Department of Defense, Goldstein argues that, "Soviet economic gains from international trade go disproportionately toward military investment that threatens the West" (p. 160). Disagreeing with the case study analysts who saw little benefit to the Russian economy from technology transfer, Goldstein sees the potential of foreign technology undermining America's military position. In essence, Goldstein fears that we are "selling the Russians the rope with which they will hang us." In contrast, other analysts argue that not selling to the East will lead to the same negative outcome.

Smith concludes with an analysis of the future of trade with the East. Trade will be limited by political uncertainties, mutual concern over trade dependence, and limitations on the East's ability to raise hard currency. On the other hand, Smith argues that trade and technology flows are important assets in American foreign policy, and can serve as an instrument to the furthering of our interests. Trade, however, will further America's interests only if used as an inducement, rather than a threat. Echoing Vernon, the control of trade through measures such as export controls and sanctions will become more difficult with time, and can only weaken the liberal trade regime, which both agree is the more critical vehicle for America's national interest.

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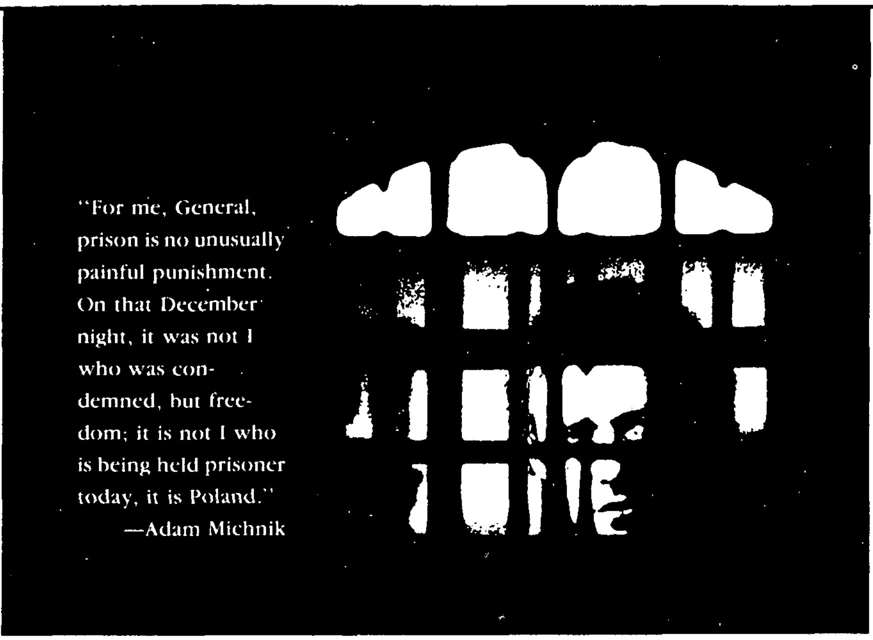

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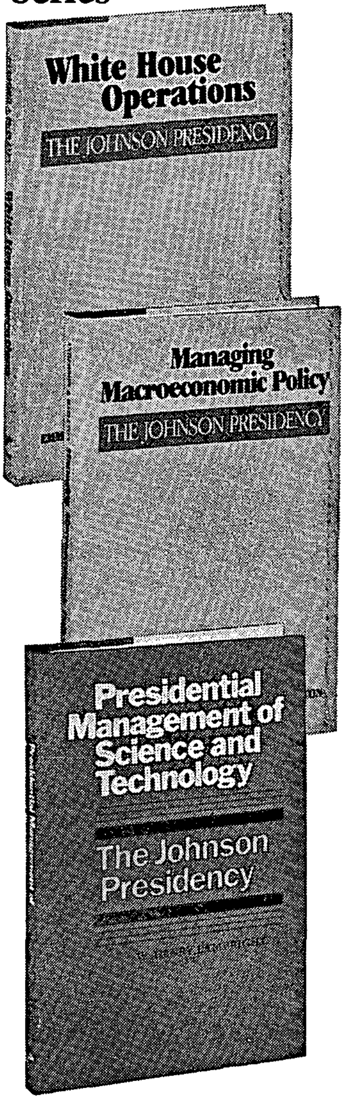

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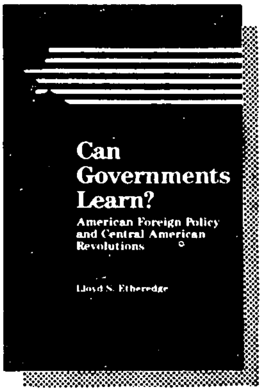

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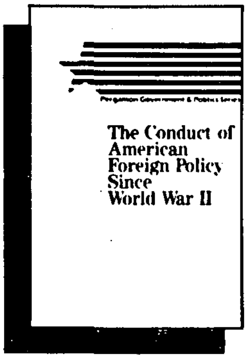

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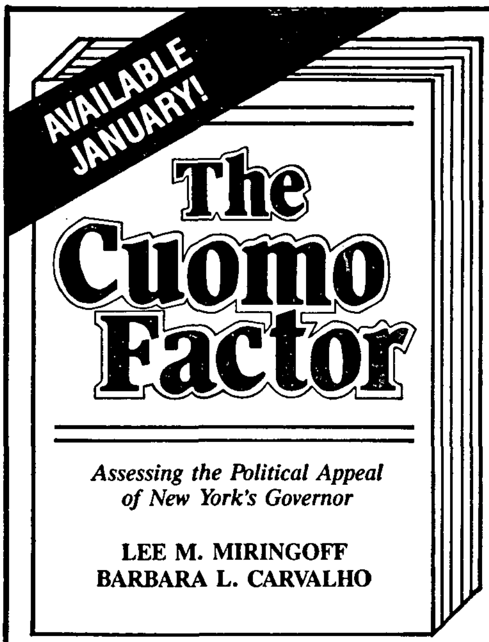

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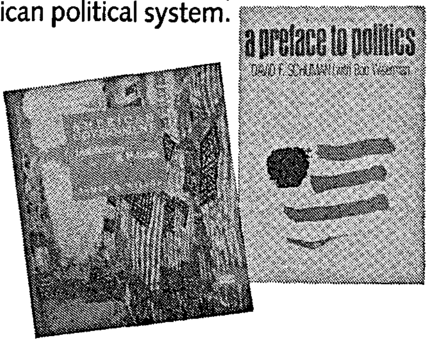

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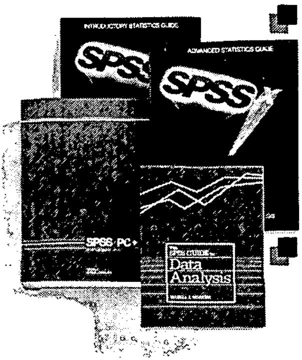

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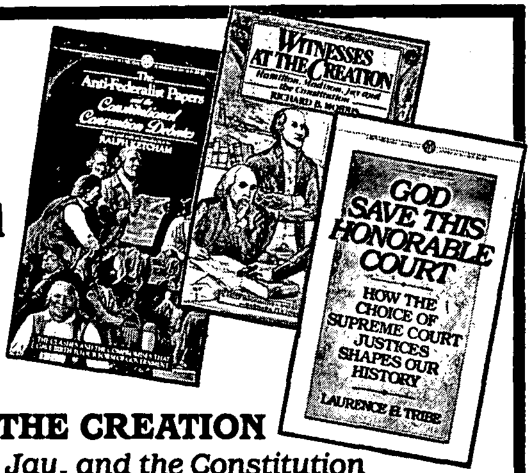
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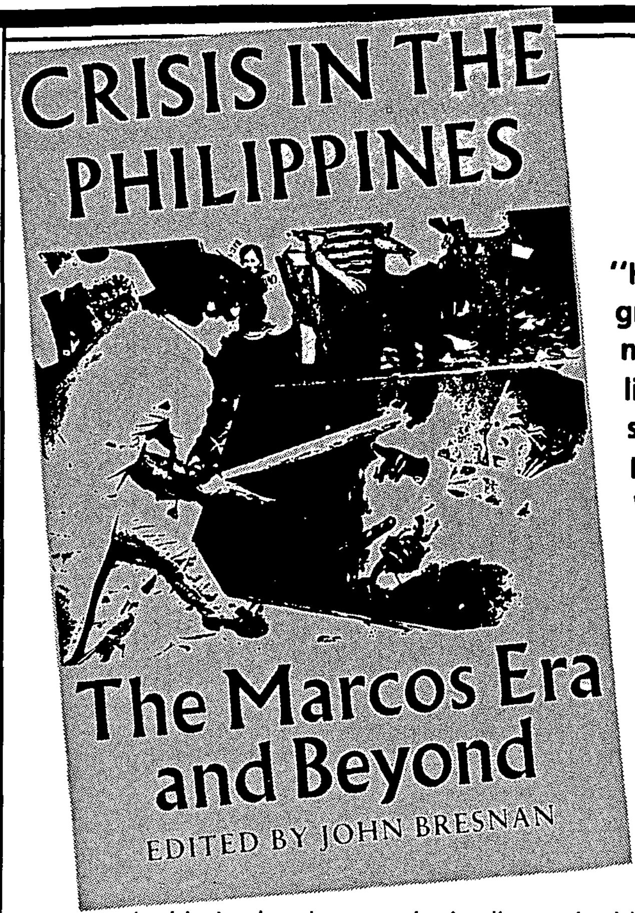

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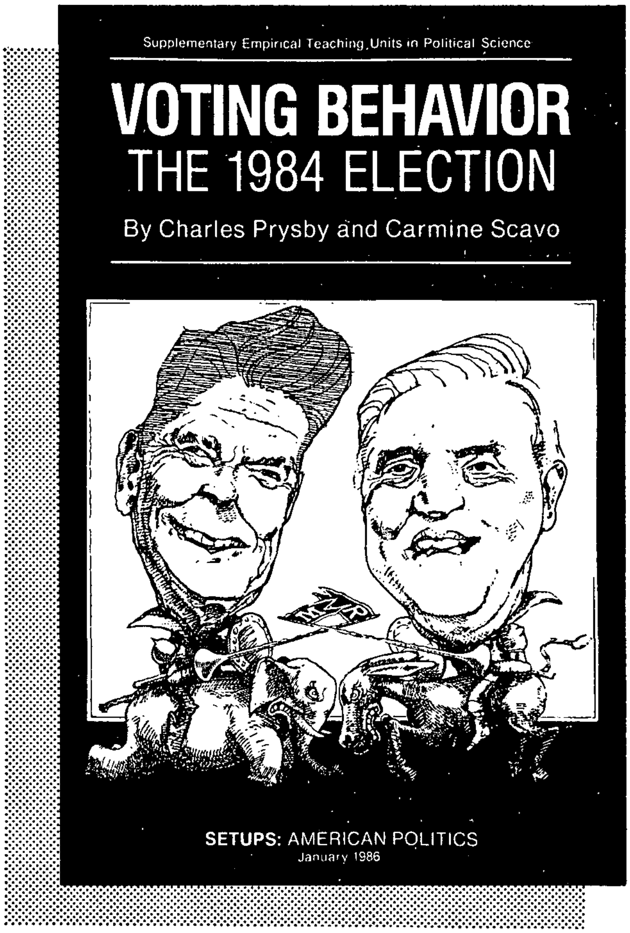

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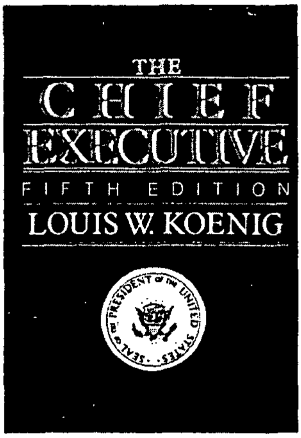
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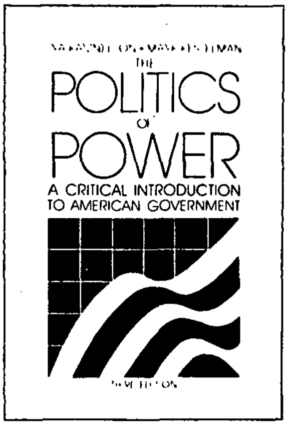
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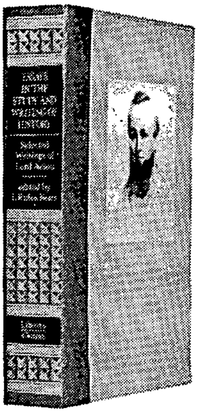

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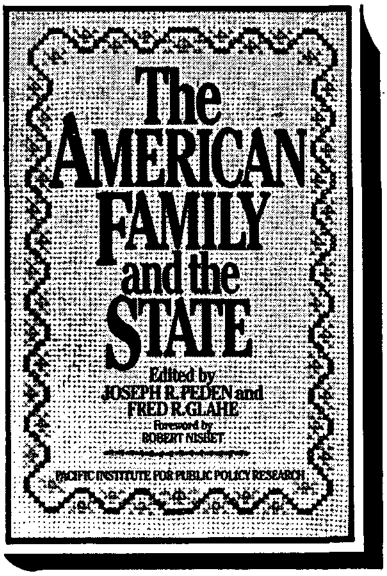

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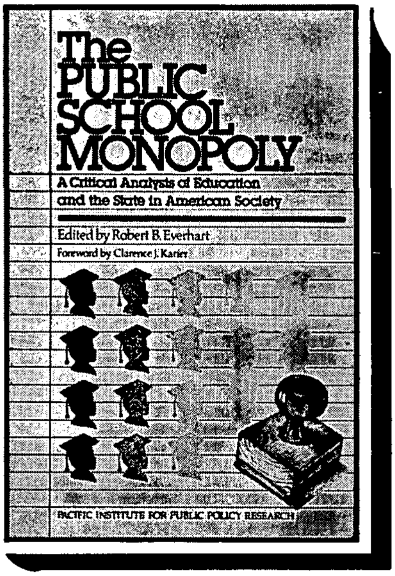

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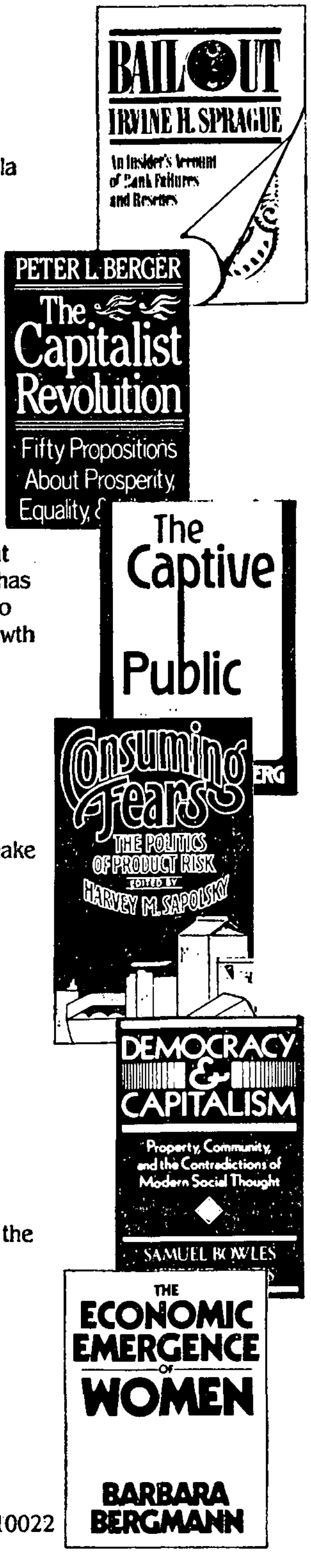
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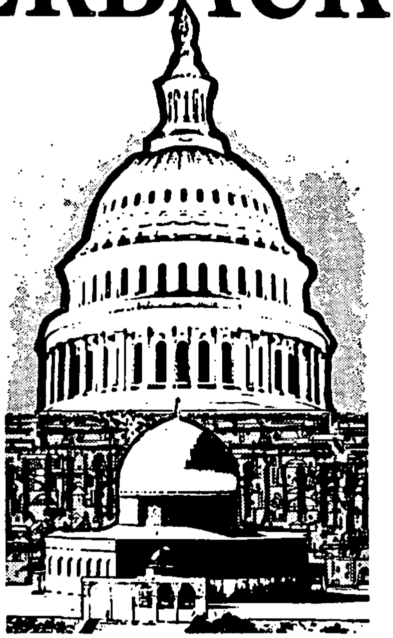

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